

J. Webb

S U R G I C A L

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J. Webb

SURGEON GENERAL



1872

S U R G I C A L
AND
P H Y S I O L O G I C A L
E S S A Y S.

P A R T II.

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J O H N A B E R N E T H Y,

ASSISTANT SURGEON TO ST. BARTHOLOMEW'S HOSPITAL;
AND LECTURER IN ANATOMY AND SURGERY.

L O N D O N:

PRINTED FOR JAMES EVANS, PATER-NOSTER-ROW.

1793.

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1843

PHYSIOLOGICAL

EXPERIMENTAL

LECTURE

BY

JOHN



ASSISTANT LECTURER IN PHYSIOLOGY
AND ANATOMY

LONDON

PRINTED FOR JAMES EARLE, PATERNOSTER ROW

1843

S U R G I C A L
A N D
P H Y S I O L O G I C A L
E S S A Y S,

B Y

JOHN ABERNETHY,

**PROFESSOR OF ANATOMY TO THE CORPORATION OF SURGEONS;
ASSISTANT SURGEON TO ST. BARTHOLOMEW'S HOSPITAL;
AND LECTURER IN ANATOMY AND SURGERY.**

L O N D O N :

PRINTED FOR JAMES EVANS, PATER-NOSTER ROW.

1793-

SURGICAL

AND

PHYSIOLOGICAL

ESSAYS



JOHN NICHOLSON

JOHN NICHOLSON

DEDICATION.

TO

MR. CHARLES BLICKE,

SURGEON OF ST. BARTHOLOMEW'S HOSPITAL.

SIR,

TO your instructions I am indebted for a considerable portion of that surgical knowledge which I possess: to your friendly exertions, I in a great measure, owe the situation of life in which I am placed. I should think myself equally ungrateful, and unjust, not to acknowledge these obligations, when an opportunity is afforded me by the presentation of these writings to your notice. I forbear to attempt any eulogium either on the good qualities of your mind, or on your professional talents; for to your friends the account were superfluous, to yourself it might
not

not be pleasing, whilst by those who know you not, it might be regarded merely as the accustomed language of dedication.

My chief design in this brief address therefore is, publicly to avow my obligations to you, and publicly to testify *my great respect* for your abilities as a surgeon, and *my great esteem* for your character as a man.

I am, SIR,

Your obliged and obedient

friend and servant,

JOHN ABERNETHY.

P R E-

P R E F A C E.

I SHOULD think myself deficient in the respect due to the public opinion, were I to permit these Essays to appear, without offering an apology for their imperfections, and defects. The reader will perceive the reasons which led me to wish the speedy publication of the first Essay; they have induced me to print it, at a time when my other occupations, prevented me from paying the requisite attention to correctness. I was prompted to undertake the experiments contained in the second Essay, because it appears necessary to introduce a course of anatomical lectures, with a philosophical account of the nature of the matter, which composes an animal body.

I design

I design to submit two other papers to the public consideration; but I shall defer their publication until the summer, when I shall have more leisure to perfect them.

I SHOULD think myself indebted in the respect due to the public opinion, were I to permit these essays to appear without offering an apology for their imperfections and defects. The reader will perceive the reasons which led me to with the hasty publication of the first essay; they have induced me to print it at a time when any other opportunity presented me from paying the requisite attention to correction. I was prompted to moderate the expressions contained in the second Essay, because it appears necessary to introduce a course of systematical lectures with a philosophical account of the nature of the matter, which constitutes an animal body.

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AN ESSAY ON THE
LUMBAR ABSCESS.
The author has the honor to acknowledge the assistance of several gentlemen, who have waited for opportunities of more frequently procuring the practice recommended in the following pages; however, as the oc-

ESSAY
ON THE
LUMBAR ABSCESS.

SOME cases of lumbar Abscess, which of late occurred at St. Bartholomew's Hospital, particularly excited my attention to that disease, and induced me to pursue in it a practice which I think will be found of considerable utility. Every improvement of the healing art ought speedily to be communicated to the public, since by its practice the sufferings of the afflicted may be relieved and their lives prolonged. The author of any innovation doubtless wishes to ascertain, by repeated trials, the nature and extent of its merits.—Experiments.

ments repeated make its degree of utility apparent and its success undoubted.—I have long waited for opportunities of more frequently prosecuting the practice recommended in the following pages ; however, as the occurrence of cases of particular disease to any individual is so fortuitous, and at certain times so rare, and as the practice hereafter related appears to me so uniformly beneficial, I have resolved no longer to delay the publication of the following cases and remarks.—Other motives also induce me to this determination ; I submitted this paper to the discussion of the Lyceum Medicum Londinense, and it gave me much pleasure to hear from several Members of that Society, that the treatment which I had proposed in these complaints, had been repeated with success in several parts of England. If the recommended practice could in all cases be perfectly accomplished, I believe it can be clearly demonstrated, that the lumbar Abscess (a disease hitherto so much dreaded) would be reduced to a complaint of little danger or importance. But many apparently minute circumstances may frustrate the intended plan of treatment. I think it right to apprise

the practitioner of the necessity of attending to these minutiae, and also to lay before the public the information which I have derived from my present degree of experience in these diseases.

Whilst the condensed cellular substance which forms the cyst of an abscess remains entire, it continues free from inflammation, and the contained pus suffers no putrefaction nor evident alteration of quality. Some lumbar Abscesses contain two quarts or more of matter. The surface of the containing cyst must in such cases be very extensive: whenever the abscess is opened either by ulceration, or by the hand of the surgeon, a sudden and generally considerable inflammation extends itself over the whole cyst; this is followed by a copious discharge of frequently fetid pus. Now this immediate inflammation and consequent discharge cannot but greatly derange and exhaust the constitution of the patient, which is generally irritable, and already much enfeebled by the efforts attending the formation of the disease.—It is well known that when we evacuate fluids from the cavities of the body, if we immediately close the

aperture through which they were discharged, we prevent the inflammation which would otherwise ensue. The evacuation of water from the abdomen, and Tunica vaginalis Testis may be adduced as instances of the truth of this remark. It is also well known, that if the matter of an abscess be discharged, its cavity becomes much diminished by the contraction of its cyst. It will hereafter be shewn that this contraction will be greater in chronic lumbar abscesses than in those of a more phlegmonoid nature, since in the former the cyst having sustained less inflammation and undergone less alteration of structure, will be more likely to possess and exert its natural elasticity, and thus greatly diminish the cavity of the abscess.

On these two observations the practice hereafter related is founded; it occurred to me that if after the evacuation of a lumbar Abscess, the aperture were directly closed and its immediate union procured, that no inflammation of the cyst would follow, which being now relieved from pressure would by its contraction and rarefaction greatly diminish the cavity:

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the pus, doubtless, would speedily reaccumulate, yet I thought by repeatedly evacuating this fluid before distention of the cyst could happen, the cavity would be so much reduced, and the cyst be made so much less extensive, that the future admission of air would be productive, comparatively, but of little consequence. Such were the sentiments excited in my mind by some of the following cases, and such were the motives which induced me to pursue the practice hereafter related.—I shall first give an account of the cases as they occurred, and afterwards offer some general observations on these diseases. To the account of each case I will annex those remarks which it suggested, and which I am unwilling to postpone to the conclusion, as they would less forcibly strike the mind of the reader if the circumstances which gave rise to them were held but imperfectly in remembrance.

C A S E.

A young man, about twenty-seven years of age, of a muscular form, and healthy constitution, came from the country to the

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hospital,

hospital, to obtain relief from a collection of matter which presented itself in the upper and fore-part of the thigh, beneath the Fascia, and immediately below Poupart's Ligament. The pain which he had previously suffered in his loins, and the impulse of matter into the tumour upon his coughing, left little doubt of the original seat of the disease. The Fascia of the thigh had yielded considerably to the collected pus, so that it did not descend so low as is common, but appeared very prominent. Although he had endured considerable pain, he had not suffered much from fever on the first formation of the abscess.

A caustic was applied on the tumour to give discharge to the matter, and three days afterwards the eschar was divided.—Eight ounces of very perfectly formed, moderately consistent, and inodorous pus issued from the incision.—The sides of the eschar now closed up the wound and prevented any further evacuation of matter. This the surgeon did not attempt to produce, thinking the delay would be useful.

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For three days no more fluid was evacuated, during which time the young man remained perfectly well, and his thigh free from inflammation.—On the fourth day the eschar became so much loosened in its circumference that part of it gave way, and eight ounces more of similar and perfectly inodorous pus was discharged. In twelve hours after this detachment of the eschar, he suffered much from fever and pain in the part, and the discharge became putrid. In two days the fever, which was of the hectic kind, seemed to be established, and from the sore there flowed a copious and increasing quantity of foetid pus. His skin was now hot, his face flushed, he sweated profusely in the night, his appetite failed him, his pulse beat 120 in a minute, his tongue was but little altered from its natural appearance, he had no sleep, and was distressingly restless.—These symptoms continued about a week without cessation, they then appeared slightly to remit, and proceeded for three weeks in the same manner, with some little diminution in their severity; his strength was now greatly exhausted, the discharge from the abscess very profuse, and in this state it

was thought right to have him conveyed into the country, where I am informed he gradually declined, and in about six weeks more he died.

This case first suggested to me the propriety of closing the opening after the discharge of the pus; for until the continuity of the cyst of the abscess was destroyed by the separation of the eschar, the part was uninflamed and the state of the circulation unaltered. The sudden inflammation and fever which followed the separation of the eschar, in my opinion, prove the ingress of air into the cavities of abscesses to be injurious. Whether this be admitted or denied, it is however sufficiently evident from this case that a large opening and much irritation of the cyst or such an abscess, is productive of the most detrimental consequences to the constitution of the patient.

That mode of treatment which causes least irritation I believe has ever been found the most successful. But whenever a permanent opening has been left in a large abscess,
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generally considerable and frequently fatal, irritation hath ensued.

It will, however, be evident in the following cases that the reduction of the abscess to an inconsiderable extent has been accomplished, without the occurrence of any local inflammation, or general fever.

C A S E.

July 1790. John Tucker was admitted into St. Bartholomew's Hospital on account of a Psoas Abscess. His health had been declining for more than three years. He had for a considerable time been an out-patient under the care of Dr. Austin, who had unavailingly endeavoured to prevent the formation of this abscess by issues made in his back, and by the administration of various medicines. He had suffered greatly from pain in his loins and fever: the abscess was very large and had descended very low on the inside of the thigh, the integuments covering it were natural, the impulse of matter into the tumour upon coughing very considerable.

His pulse was feeble and beat eighty-six in a minute, previous illness had exhausted his constitution; he had a constant cough, and undoubtedly much diseased lungs.—He had little appetite and was of a costive habit—he was of fair complexion, light hair, and blue eyes, and his countenance frequently flushed:—He was on all these accounts as unfit a subject, as can well be supposed, to encounter the derangement of constitution, which must succeed to the ordinary evacuation of the abscess.

On Wednesday the 28th of July, I tapped the abscess with a small hydrocele trochar and discharged three pints of pus of good quality, although in a small degree more fluid than common. I dressed the part with considerable caution. I moistened the lint which I applied to the orifice with tinctura benzoës composita, over this I applied some sticking plaister, which was retained by cross slips, and afterwards varnished over with gum; some compresses of linen were applied over the abscess, and gently bound on by a flannel roller.

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On Thursday, there was no very perceptible difference in his health—he had slept and eat as usual, his tongue was moist and natural, his pulse a few strokes quicker.

On Friday, he said, that he found his loins relieved by the evacuation, that he could perceive no difference in his health, and his pulse was the same as before the operation. For many days his health remained unchanged, he became he thought a little weaker, and the frequency of his pulse had encreased about four strokes in a minute. For this little alteration we could readily account, knowing that some fluids were drained from the circulation into the cavity of the abscess, and that some little exertion of the system would necessarily ensue.—The abscess remained without pain, or inflammation, and his constitution free from fever; his skin continued in its natural state, his appetite was good, his sleep sound, and his countenance unaltered. Three days after the operation I removed the dressings from the punctured part, it appeared healed; I however carefully renewed the dressings every third day.

Friday,

Friday, the 13th of August, sixteen days after the first discharge, the tumour having become prominent, I again punctured it and evacuated its contents. I knew the discharge would encrease his weakness, yet, if the collection were suffered to remain it would shortly distend the cyst to its former dimensions, and my original plan of treatment would be frustrated.

The quantity of the discharged fluid was nine ounces ; in appearance and chemical properties it much resembled blood. This bloody effusion was probably the consequence of laxity of the exhaling vessels, as there had not been the least expression of inflammation in the abscess. Before I discharged the matter the second time he complained of some pain in his loins ; but the following day he said he was much relieved, and found himself remarkably well. This second puncture was dressed like the former and quickly healed.

During the time which had elapsed between the first and second discharge, he had not been confined even to the ward, but often
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went from the hospital to see his friends. This his cough, the weak state of his health, his disinclination to live in the hospital, and the obvious impunity with which it was done, induced me to permit. After the second evacuation he altogether lived with his friends, promising to come every week to let me see the state of his complaint; however, the second week when the matter ought the third time to have been evacuated, he failed in his promise. I was now obliged to leave London for some time, so that I did not see the patient again until September the 8th, which was four weeks and five days from the former evacuation; he had refused to have the matter let out during my absence. I now discharged in like manner ten ounces of lymphatic exhalation, rather dark coloured and turbid, as if mixed with true pus. The man, during the last week, had complained of pain in his loins and in his knee, both of which were relieved as usual by the operation.

Before the abscess was first opened the impulse of matter from the loins, on coughing,
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was extremely forcible, but was now not at all perceptible. It appears to me that a very considerable advantage is derived from this mode of treating these complaints. Whatever secretion is made in the abscess of the loins, will, by its gravity, descend into the space left by the seceded fascia of the thigh. The abscess of the loins being left perfectly free from distention will most probably contract to very little dimensions, if it be not perfectly abolished. Hence in the subsequent treatment of these complaints you have only to attend to the disunited fascia; whilst the cavity in the loins scarcely deserves notice.

September 22d, a fortnight after the former evacuation, I discharged four ounces of similar serous fluid mixed with pus. During its evacuation, which was very speedy, I had applied my fingers beneath Poupart's ligament, as if to obstruct the descent of any matter from the loins. I then desired the man to cough, but no matter descended, and the collection appeared to me entirely confined to the thigh.

I found

I found some difficulty in introducing a trochar, when the abscess contained so little fluid. This was remedied by first introducing a lancet through the fascia, and then conveying the trochar through the aperture made by the lancet.

Thus after discharging the matter four times, the complaint was reduced from a lumbar abscess, containing three pints, to a small collection of matter beneath the fascia, containing four ounces.—What communication this had with the loins, and what was the state of parts there, cannot be determined. To appearance there was no collection. If I had now immediately opened the abscess, the containing cyst being small, the inflammation probably would not have been considerable. But the state of the man's health induced me for a short time to defer this final attempt, this radical cure, as I may express it, and be contented with only evacuating the matter when collected, without suffering the collection to increase the size of the cyst. It might be expected, by repeating the evacuation, that the cavity would diminish to its

its total abolition. This would probably happen were the abscess in the cellular substance; but the inelastic fascia cannot contract, and the subjacent muscles cannot be elevated, so that the effused matter, though very small in quantity, would still preserve them disunited.

I had let out four ounces of matter once in October, and on the 5th of November I opened the abscess by an incision about an inch and a half in length at the lower part. I introduced my finger beneath the fascia as high as Poupart's ligament, I desired the patient to cough, but no matter descended from the loins, neither could I ascertain any communication. The extent of the detached fascia was about four inches and a half in length, and nearly four in breadth. The cyst inflamed after opening. The hardness and quantity of the discharge encreased for four days and then gradually subsided. His thigh was stiff and sore, so that he could not easily move it, but he had no particular pain in his loins—his pulse did not vary—his tongue was not furred—his sleep was not interrupted—nor could any derangement of his health be perceived.

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Granulations grew from the edge of the incision, and the opening nearly closed and afforded scarcely any discharge.—Yet, on introducing a probe through the orifice, I found that the fascia remained difunited. With a view to produce an union, by exciting inflammation, I introduced a seton from this lower orifice to the upper part of the cyst. The fascia again inflamed, indurated, and united, only the track of the seton was unclosed; and this by the injection of some spirit and water, was also soon induced to fill up. In discoursing with the patient, after opening the abscess, respecting his health, he said, he was ten times better than before it was opened; that until this time he had always been subject to fits of pain, and to a state of weakness and faintness which he could not describe.

After the perfect closure of the abscess, he could extend and bend his thigh with freedom and ease; he could also readily put his foot upon a chair set before him. This it would have been impossible for him to accomplish during the formation or continuance of the abscess. This freedom of action in the psoas

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muscle indicated considerable soundness of it, and of the contiguous parts. He still, however, complained of much rheumatic pain in his hips and sometimes in his loins; and as I supposed his constitution might be affected by the suppression of a long-continued purulent discharge, and might attempt for its relief the formation of a new abscess, I inserted two setons in the integuments of the loins, with a view of preventing inflammation of the internal parts.

They did not, however, relieve his pains; he complained much of their inconvenience, and as he designed to go into the country, they were discontinued. I saw him about a year afterwards—no alteration had taken place in the thigh, nor no fixed pain had attacked the loins, but he was still much teized with unsettled rheumatic pains.

The preceding case was very unfavourable both from the patient's constitution and from the degree of the disease. Yet, by four times discharging the matter, which was not attended with much more pain than bleeding,
it

it was reduced from a lumbar abscess, containing three pints, to a small collection beneath the fascia of the thigh, containing four ounces, and without any evident communication with the loins. Each time, instead of suffering inconvenience, he experienced relief; he had no fever, neither was he restrained from his usual occupations.

The final opening might have been sooner made, but as this was the first case in which I had pursued this practice, I was uncertain of the event and irresolutely protracted it for two months, hoping the amendment of his health. When it was opened no perceptible fever followed, and it shortly got well by the treatment which I have related.

C A S E.

Isaac Dean, thirty-seven years of age, had come from Hampshire to London, to obtain advice for a Psoas Abscess. He was admitted into the hospital under the care of the late Mr. Pitts. The account which he gave of himself was, that his business had obliged him to be much on horseback; that he

had formerly, when riding, bruised his left testis, which accident had occasioned an incurable disease of that gland; he therefore had suffered its removal about two years since in some county hospital. Since that time he had frequently suffered much pain in his loins; about eight months before his admission into the hospital he had caught cold: the pain in his loins then became more violent and constant, and much impeded the motions of his left thigh. About three months after this attack of severe pain, he perceived a tumour in the upper part of his thigh, which had gradually encreased until the time of his admission into the hospital. Since the appearance of the tumour, the pain in the loins had much abated. The matter now descended about four inches beneath Poupart's ligament; and it received a forcible impulse when the man coughed. The fascia, containing the descended pus, was very prominent, and the skin covering it was more red than the rest of the integuments.

The patient's health was not unfavourable; his pulse was rather strong, beating seventy-six

fix in a minute, his tongue rather pale, his hair and eyes dark.

Monday, 3d of October, 1790, by Mr. Pitt's desire I introduced a trochar into the lower part of the tumour, and gave discharge to twenty-four ounces of pus, moderately tenacious, and containing some flakes of firmer matter: I cautiously closed the orifice, as in the former case, applied a compress, and bound it moderately tight with a roller.

I could not in this case perceive any alteration in the man's health deserving to be recorded, except that the pulse was a little quickened: he eat and slept as usual.

I carefully took off the sticking-plaister at the end of three days, and renewed a similar dressing. On Thursday, 13th of October, the abscess was now again prominent, and the puncture made by the trochar seemed slightly inflamed. As I concluded the distention of the fascia caused this inflammation, and supposing that if the pressure of the matter from beneath was suffered to continue, it might

occasion it to ulcerate, I determined to prevent this effect by again evacuating the matter. This I accomplished by passing a trochar into the lower part of the abscess, at some distance from the former opening; and by this means discharged between eight and nine ounces of pus, thinner and rather darker coloured than the former, but not tinged with blood as in the preceding case. I now carefully dressed both orifices, and again applied a bandage.

I cautiously removed the dressings, at the end of three days; the second puncture had healed, and the first had lost its disposition to inflame. After having dressed the punctured parts, and applied the bandage; I desired him to moisten it with aq. saturn. which I thought by keeping the skin cool, would prevent its disposition to inflame. The man suffered no alteration in his health from this second evacuation. On the 25th, at the end of a fortnight, the tumour being again prominent, I introduced a lancet into the fascia, and by the orifice thus made the trochar, and discharged six ouches of turbid serous fluid, and pursued

sued the same subsequent mode of treatment.

After another fortnight had elapsed the tumour was much less prominent than before, and there appeared a degree of irritation in the skin. The punctures shewed a disposition to inflame. I now desired the man to cough, but could discover no impulse of matter from the loins. This I had not before done, least the exertion should affect the punctures, which were not so firmly healed as in the former case. As the patient had not suffered much from discharge, as his health seemed fully capable of sustaining the effects arising from opening the abscess, as it was not probable that its dimensions could suffer further diminution by delay, on Friday, the 23d of November, I opened the cavity by an incision of about an inch in length, at the lower part, and immediately passed a seton through to the upper part, with a view to insure the union of the fascia.

An usual degree of inflammation of the fascia and stiffness of the affected limb fol-

lowed, but he complained of no particular pain in his loins further than general stiffness. The slight fever which accompanied seemed rather inflammatory than hectic, his pulse became a little quicker and harder, and his tongue slightly furred. These symptoms gradually abated, and at the expiration of three weeks the fascia appeared to have adhered firmly to the subjacent parts: I therefore withdrew the seton,

As he now found his health tolerably good, and being, as he thought, recovered from what he considered as a dangerous complaint, and imagining that he was made weaker by staying in the hospital, he went into the country, promising to inform me if any change happened; but I have not since heard of him. Two cases of such remarkable success, established, in my opinion, the excellence of this mode of treatment; my expectations of future success were sanguine; and although they have not been completely fulfilled, yet every succeeding case has tended to demonstrate the utility of the method.

C A S E.

C A S E.

July, 1791. A poor woman, fifty-three years of age, had in the beginning of March, a severe pain in her loins, which gradually abated on the appearance of a tumour on her back; this continued to enlarge until her admission into the hospital, which was in July. A small tumour was also perceived beneath Poupart's ligament. When the patient coughed, matter was forcibly impelled into both swellings; so that there was little doubt that there a lumbar abscess; the matter of which had made its way through the muscles of the back and formed the principal tumour beneath the integuments.

Friday, July 18, Mr. Earle punctured the abscess in the back with a lancet, carrying it for some distance obliquely, between the skin and the cyst of the abscess; so that the orifice of the integuments did not correspond with the orifice of the cyst: thus a kind of valve was made, which it was supposed would prevent the ingress of air into the abscess. Seven ounces of good pus was discharged, and the

the pucture was attentively dressed. The following day the abscess appeared as large as before. This confirmed the opinion that it communicated with some internal collection. On Monday it was less distended; but on Tuesday, being very full, and fearing least the pressure of the contained matter should cause an ulceration of the former orifice, I punctured the abscess as before, and discharged five ounces of thick pus; such as could not be supposed to have been formed in so short a space of time. She was not in the least disturbed in her health, and both orifices healed immediately without trouble.

The following week, on Tuesday the 2d of August, I again punctured the tumour and discharged between four and five ounces of pus. And again, on the succeeding Tuesday, I discharged in like manner three ounces of matter. On the Tuesday afterwards, as there did not appear any reaccumulation, I made an incision into the tumour of about one inch in length. About two ounces of fluid issued from the incision; the cavity of the abscess I found larger than I expected, but I could not perceive

perceive any aperture by which it communicated with the loins. On Wednesday the patient appeared well; her tongue was moist, her pulse a little more than seventy in a minute, and had a common degree of fullness and of strength.

On Thursday she was in the same state with respect to her pulse and appearance; but she was in very low spirits. She wept much, and said her back was painful, but did not otherwise complain. On Friday, business prevented me from going to the hospital, so that I did not see her. The pain in her back and the depression of her spirits had much increased; her hands were cold, and her pulse was much quickened: in this state Mr. Harvey saw her, and directed her wine and other cordials; but she soon died.

I opened the body, and it appeared, that the abscess had been originally formed on the posterior surface of the psoas muscle. This had now contracted into a kind of tubular cavity, the diameter of which was about one-sixth of an inch; and extended from nearly
opposite

opposite to the last dorsal vertebra to beneath Poupart's ligament, at which place the abscess had presented itself. From the upper part of this cavity a passage was seen on the inner edge of the quadratus lumborum muscle, by which the matter had escaped from the loins through the muscles of the back, and had afterwards elevated the integuments, causing the tumour formerly described. I could not observe any appearance of inflammation on the inside of this cavity; indeed, as only two days had intervened between the opening of the abscess and the patient's death, the time seems insufficient for the establishment of a disease of this nature.

Almost every surgeon has met with cases in which the nervous system has been so circumstanced, as to be incapable of sustaining the shock of an operation, or of attempting to remedy a disease; and in such cases the patient has suddenly perished without any evident cause. In the present case there was nothing indicating a peculiarity of the nervous system; the pulse was seventy, and in other respects equally natural: she was rather

ther a weakly woman, but she eat and slept as usual; every thing succeeded till the final opening of the abscess: there was every reason to suppose it was greatly reduced in size, and dissection confirmed the opinion. I know not that the event of this case could possibly be foreseen. One inference only can I draw from it, which is, that whenever any debility of constitution is perceived, one ought as long as possible to delay the final opening, or until that debility be removed.

C A S E.

February, 1791. James Leaver is in the 21st year of his age, has light brown hair, blue eyes, dilated pupils, pale countenance, frequently flushed, and is apparently of an irritable constitution. About nine months ago he was affected with a pain in his loins when he moved, which soon became very severe, even when he was at rest. This pain was accompanied with fever. Four months afterwards he perceived a small swelling in the upper part of his right thigh, which has since gradually encreased, and has now descended

scended nearly to the middle of the thigh : he remarked, that he never had the least pain in the part where the tumour was formed. After the appearance of this swelling, he no longer experienced the same degree of uneasiness in his loins ; and shortly after, he acquired the power of lifting up his right thigh, which he had for some time lost.

For four months previously to his admission into the hospital, he had regularly profuse night sweats, which began about twelve o'clock, but did not prevent his sleeping ; when he awoke he found his cloaths very wet, and himself very chilly ; he had, however, an appetite for his breakfast.

On the 5th of February, Mr. Earle introduced a trochar into the most prominent part of the tumour : between two and three pints of healthy matter was evacuated, the wound was immediately closed, and lint and adhesive plaister were applied. The night succeeding the operation he slept little, but was free from perspiration. On each succeeding night he slept as usual, but had not in the least degree
those

those sweats which had been constant until the discharge of the matter.

On the 8th of February, he said he found himself no worse for the operation, he was free from night sweats and slept soundly. His appetite was perfectly good, his bowels unaffected, and his tongue moist and florid. His pulse, before the operation, was ninety, and for fifteen days afterwards it varied between that and a hundred. February 15th, ten days after the evacuation, his night sweats returned, although in a less degree than formerly.

February 26th, three weeks after the first discharge, the tumour had now become nearly of its original size; the integuments were much distended; the part punctured by the trochar had for three days appeared inflamed; and on the tumour being now compressed, the cicatrix gave way, and the contained matter oozed from the orifice. The trochar was again introduced through the former orifice, and eight ounces of brownish matter discharged. The wound was carefully

fully dressed, in hopes that as the distention was taken off, it might close. After the second evacuation, the night sweats were again discontinued; he said, he was rather weaker, but no other alteration in his health was perceived.

On the 2d of March, while in the act of coughing, the imperfectly healed wound made by the trochar, gave way. Very little pus was discharged, but as it was impossible to heal this ulcerated opening, and as the continuity of the cyst was now destroyed, the mode of treatment hitherto pursued was frustrated. Much inflammation of the cyst immediately took place, and the constitution became greatly affected. The next day, if the finger slightly compressed the abscess, it gave him great pain; but before the cavity of the abscess became exposed, the part was perfectly indolent. When pressure was employed, a foetid, frothy matter issued from the ulcerated orifice. The cyst, however, was emptied, and, except when pressed, there was no discharge. Such were the appearances of the part. The general disturbance
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of the constitution was also very great ; his countenance exhibited strong expressions of alarm ; if any one approached him he started, and when any one touched him he trembled. His pulse beat from 130 to 140 in a minute—for two days his bowels were disordered—however, the inflammation of the cyst gradually abated, and in like manner the constitutional derangement subsided. At the end of about eight days, he was much amended, and in about six weeks the abscess appeared nearly well, and his constitution relieved from febrile indisposition.

In this case it is clear, that the second discharge of matter was too long delayed, and to me it appears equally evident that the patient derived much advantage from the mode of treatment which had been pursued ; for by it the complaint was reduced from a large abscess, containing nearly three pints, to one which held less than eight ounces. Yet, even in this diminished state, great derangement of the constitution followed the exposure of the cavity of the abscess : indeed, I have little doubt, if the abscess had been

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opened whilst it retained its original dimensions, but that the patient would have fallen a victim to the more extensive inflammation, and more violent fever, which would then have taken place.

C A S E.

Elizabeth Ridley, aged fifty-five, had for one year and a half before her admission into the hospital, suffered much from bad health, she then had a severe cough, accompanied with much fever. About ten months ago, she had a very acute pain in her loins, which abated, in some degree, ten weeks after its first attack; at that time she observed a tumour in her groin, which had gradually increased in size. The pain had been continued, though at intervals it suffered considerable abatement: the veins on the fore part of the thigh had become varicous and the leg œdematous. The tumour was of a circular form, about four inches in diameter.—It had much protruded the fascia, and matter was violently impelled into it on coughing. She now complained of occasional pain of
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her stomach, of failure of appetite, and a costive state of her bowels; her pulse was slow and feeble, her tongue pale, and her health considerably beneath the natural standard.

On the 8th of November, I punctured the lower part of the tumour with a lancet, carrying it obliquely about half an inch between the skin and the fascia, and discharged eleven ounces of good pus, but did not empty the abscess. The orifice of the skin and cyst did not then correspond, and on coughing there was still perceived a considerable impulse of matter from the cavity in the loins.—I was unwilling to irritate the cyst by the introduction of any instrument to separate the lips of the wound, therefore I closed the orifice with sticking plaster, and every thing remained quiet till the third day, when, by a fit of coughing, the orifice was burst open and matter oozed from beneath the plaster. If I suffered it to remain open, my original plan of treatment would be frustrated. I therefore resolved to let out the collected matter, least distention of the fascia and integuments

should prevent the wound from healing. I again introduced the lancet through the same orifice, and wounded it so as to make it bleed and gave a discharge to five ounces of pus; the abscess though, did not even now appear to be completely emptied. I preferred to introduce the lancet through the same orifice rather than make another opening, that this new injury might excite in the divided parts a new disposition to unite. If I had not again made the separated parts bleed, they probably would have united by granulations; their surfaces would have been for some time kept separate by a purulent secretion, and air would have been admitted into the cavity of the abscess: but the effused blood glued together these edges, and thus obstructed the aperture till its organization made the reunion perfect.

The woman suffered no evident alteration in her health, but became much easier with respect to her loins. The varicose veins and the oedema of the leg now no longer appeared. These symptoms, doubtless, originated from the pressure of her loins, occasioned

fioned by matter, of which it was very evident there was a large collection.

On the 18th, the tumour was again punctured and eight ounces of fluid evacuated. The matter before had been incompletely discharged; now I believe the tumour was entirely emptied. This last discharged matter was perfectly inodorous and the thigh uninflamed. I made this aperture at the side of the tumour with the edges of the lancet held upwards and downwards, and not transversely as the former openings had been made. This I did that the efforts employed in coughing might have less effect in impelling the matter through the orifice, which soon healed.

In the following week she complained that she was restless and could not sleep, neither had she her usual degree of appetite; her pulse, however, was not quickened, nor did any other signs of constitutional indisposition appear. No matter was now collected beneath the fascia, and after waiting another week without any apparent collection being made, on the 25th of November I intro-

duced a lancet through the fascia of the thigh, with a design to admit the air into the cavity of the abscess that remained. I did not perceive any matter issue from the opening. As the integuments covering the fascia were thickened and shewed some disposition to inflame, I directed the aqua aceti lithargyrita to be applied to them. On the following day some matter flowed through the orifice. The patient supposed, if collected it might be a table spoonful; nearly the same quantity continued to discharge for about a fortnight, and afterwards it gradually diminished, and the wound healed. She was not affected by fever in consequence of this last opening, and seemed to suffer very little inconvenience with respect to her health. She, however, complained much of pains resembling those of the rheumatism, which affected principally her hips, though sometimes they attacked her loins; for these pains she was placed under the care of the physician, and as her constitution was languid, she was recommended to continue the medicines prescribed for her as an out-patient.

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In this case one circumstance appeared to me curious ; after I had twice discharged the contents of the abscess, no further collection of matter took place. Yet not because the cavity of the abscess was abolished, but because from some little indisposition of the constitution the secretion into that cavity was for a time suspended. This, however, was rather an advantageous circumstance, for as the cyst was without repletion, the contraction of the fides was unopposed.

There were also three other patients in the hospital, from whom the matter had been twice discharged, and the dimensions of the abscess considerably diminished ; yet when the abscesses were punctured a third time, the wounds either ulcerated, or did not unite ; so that the complete reduction of the abscess was prevented. As I had not these cases under my own management, and as the plan of treatment was not perfectly executed, I shall not engross the reader's time by the relation of circumstances which do not, in my opinion, contain any useful information. The account which I have given, comprises my present experience in these complaints: in

every new undertaking unexpected circumstances will occur, which will often baffle and sometimes defeat its intention. The difficulty of uniting the wounded parts has, in some cases, frustrated the intended treatment. Particular attention is required to prevent the rupture or ulceration of the punctured parts. In one case the final opening ought to have been longer delayed: when these circumstances shall be attended to in future, there is, I think, reason to believe that our success will be more complete.

I shall now offer some general remarks on the lumbar abscess, not, however, desiring to be systematic, or to comprehend every particular; but to notice principally those circumstances, which have a reference to the practice pursued in the foregoing cases. The remarks which I design to offer will be arranged under the following heads:

1st. I will describe the appearances of these abscesses, as I have observed them in dissection, with a view to establish my opinion of their nature,

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2dly. I will endeavour to explain the difference between these and plegmonoid abscesses.

3dly. The treatment appropriated to their different stages will be considered. The reason why these abscesses so rarely disperse, and so frequently enlarge to such extensive dimensions will be investigated.

4thly. The cause of the local and constitutional derangement which succeeds to the ordinary evacuation of the abscess will be enquired into ; and it will be attempted to be demonstrated, that the lumbar abscess is dangerous only from its magnitude.

5thly. It will be shewn, that the proposed mode of treatment reduces its dimensions to a inconsiderable extent, and proportionably detracts from the hazard of the patient.

And lastly, the principal circumstances conducing to render the treatment successful, will be stated.

It has been, I think, too prevalent an idea,
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that the psoas abscess frequently is connected with and often causes a caries of the bodies of the vertebræ. This opinion requires examination. I have formerly seen many bodies opened in which these abscesses have been found. I will describe the appearances of their formation in the loins, and their progress to other parts of the body, from my own observation.

The cellular substance, interposed between the peritoneum and the loins, is the common seat of these abscesses; this substance is in greater quantity at the sides, where it connects that membrane to the psoas, and quadratus lumborum muscles, than in the middle when it attaches it to the spine. Where this substance is most abundant there most frequently are abscesses formed; and this probably is the reason why we generally find these suppurations limited to either side of the vertebræ, and seldom extending across them. If matter was formed in the middle, opposite to the bodies of the vertebræ, its gravity and the want of resistance would determine its descent to either side. As the peritoneum would readily yield to the protusion of matter collected

collected behind it; as the cellular substance connecting it to the vertebræ would be easily separated; so the pressure which the collected pus would make against those bones would be quite insufficient for the production of disease. The matter of such an abscess is also perfectly mild, and could not stimulate except by its mechanical properties. I therefore think it improbable that a caries of the bones should be the consequence of an ordinary lumbar abscess.

Sometimes the abscess is formed in the cellular substance behind the psoas muscle, and then perhaps it most frequently makes a passage towards the integuments of the back. This happened in the fourth case which I have related.

But the most frequent situation of the abscess is before or by the side of the psoas muscles, from whence the fluid collected sometimes extends itself laterally, and making its way between the three strata of abdominal muscles presents itself beneath the integuments of the abdomen. These abscesses are gene-

rally regarded as less dangerous; perhaps the complicated course which they pursue prevents the ready propagation of inflammation to the original cavity after they have ruptured or are opened.

But the gravity of the matter, and the yielding state of the cellular substance generally determine it to descend with the psoas muscle beneath Poupart's ligament, in which situation it elevates the fascia of the thigh. This is the most common progress and presentation of the lumbar abscess.

Some cases are on record where the matter has descended in the cavity of the pelvis, and through the upper part of the obturator foramen, or through the great sacro ischiatic foramen, or has protruded the integuments by the side of the rectum. Such is the original situation, and such the various presentations of the matter of a lumbar abscess, which I believe very rarely causes any disease of the adjacent bones.

That pus may be collected in the loins, in
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consequence of carious vertebræ, that it may accumulate, extend, and present itself like a lumbar abscess, is, I believe well known; but here the disease is complicated. When such an abscess bursts, the patient's life is in imminent danger; his constitution is now obliged to encounter all the irritation attending an inflammation of a very extensive cyst, and to support a copious, and constant purulent discharge, at a time when it has been enfeebled by a prior and very considerable disease. I have, however, seen patients escape the immediate hazard arising from the rupture of such an abscess, I have seen them with openings in the thigh discharging matter, when the vertebræ have been carious, and the limbs in consequence palsied.

To these cases the practice which I have proposed seems peculiarly adapted. The accumulated matter would probably make a spontaneous outlet, and the destruction of the patient would be the almost inevitable consequence. But the temporary evacuation of matter which I have recommended, obviates distention and prevents this effect. Whilst
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the art of the surgeon is employed in remedying the cause which produced the matter ; in detracting from the disease of the corpora vertebrarum by external derivation by issues made in the back, of the utility of which means we are assured by long continued experience.

I believe I cannot give a more useful or intelligible account of these diseases than by tracing them through their various stages, and by remarking the symptoms, and treatment appropriate to each. These complaints present us with a specimen of languid chronic inflammation, terminating in an abscess. Sometimes this is effected with little pain, and though the patients usually complain much, during the continuance of the inflammation, yet their sufferings are by no means so severe, as those, which attend on a phlegmon. Both the rapid and violent inflammation, which is called phlegmon, and this more languid and chronic inflammation may terminate in suppuration, and produce an abscess ; but the circumstances of the surrounding parts are very different, and to these I wish for a short time to direct the attention of the reader.

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In phlegmonoid tumours the violent inflammation which causes suppuration in the center gradually abating in the circumference, occasions there, adhesion and thickening of the cellular substance; so that there are two causes of the confinement of pus in a phlegmonoid abscess, viz. condensation of the surrounding cellular substance, which is the consequence of the pressure of the collected matter; and a thickened unnatural state of that substance which is the effect of the degree of inflammation, that it has endured. When, therefore, a phlegmonoid abscess is opened, although, as is well known, the sides will greatly approximate to each other, and the cavity will be much diminished; yet, the contraction will be less complete on account of the diseased state of the sides of the abscess. But if the abscess be the consequence of chronic inflammation, the surrounding parts take very little of the disease; these, therefore, are but little altered from their natural state, and the collected pus is confined almost solely, by the condensation of the surrounding substance into a cyst. It therefore appears that the recommended practice is particularly adapted to these cases;

eases; as when the evacuation of the matter diminishes the condensing cause, the surrounding cellular substance is likely, in some degree, to regain its original rarity and looseness of texture, and thus greatly to diminish the cavity of the abscess.

In abscesses, formed in consequence of acute inflammation, as the surrounding parts partake of the affection, those which intervene between the matter and the surface of the body, soon inflame and ulcerate, and thus the collected pus quickly obtains a spontaneous outlet. But in the chronic abscess the surrounding parts have little disposition to inflame, and the approach of matter to the surface is therefore much more tardy. When it descends, as usual, beneath the fascia of the thigh, there is an additional cause, why the skin is not readily affected; the matter confined beneath the resisting fascia cannot irritate it by its pressure.

I will relate the case of a woman who was late in the hospital, as it well illustrates all the circumstances of the formation and progress of these chronic abscesses. An accumulation
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of twelve ounces of well-formed pus happened beneath the integuments covering the upper part of the pectoral muscle; it had elevated the skin, and had formed a globular kind of tumour. This suppuration had been attended with scarcely any pain, and the integuments, although distended, were indolent, and appeared perfectly healthy and natural. I punctured the abscess with a lancet conveyed obliquely between the integuments and the cyst, evacuated the contained pus, and closed the aperture with sticking plaister: but on the re-accumulation of matter it was no longer confined in a cyst, but had diffused itself through the cellular substance leading to the axilla, in which inflammation was produced. I was, therefore, obliged to make a new orifice, and leave it open, that the secreted matter might have an outlet, and not extend disease, by thus pervading the cellular substance.

The lumbar abscesses generally occur in unhealthy subjects, but they sometimes are found in people who possess considerable strength. Any thing which can induce inflammation in the cellular substance may give rise to them. Their origin is often to be

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traced

traced to accidental colds, to strains, and to febrile diseases inductive of inflammation. Doubtless the inflammation is sometimes more acute, sometimes more languid, as the producing cause, and the constitution of the patient determine. If matter be once formed its presence produces an increase of disease, it presses on and gently irritates the surrounding parts in a manner likely to perpetuate this chronic inflammation. In consequence of such irritation more matter is deposited; and the greater the accumulation of pus the greater is the stimulus to the surrounding parts, and thus the disease is augmented.

Pain in the loins is the common attendant on fever, and on weakness, when no inflammation is present. This circumstance, perhaps, makes us too little attentive to such complaints, from our patients. An inflammation may at a certain period have begun, it may now be checked, and many a lumbar abscess might probably be prevented by timely attention. The means by which this purpose is to be accomplished, either operate on the constitution, or on the affected part. The former

former comprehends such evacuations from the circulating system as the strength of the patient will permit, and other means which it would be unnecessary to mention since they are familiarly known to every surgeon. Of local treatments, scarifications, blisters, and setons in the loins, promise advantage by deriving inflammation to the surface.

Confinement to bed and perfect quietude should be enjoined. Whenever the patient sits up, or puts his thigh in action, the inflamed parts must be injured by motion. An impediment to the motions of the thigh does in some degree indicate the presence of inflammation in the loins, and a consequent affection of the psoas muscle. This symptom often goes off when suppuration is formed, but frequently the inability to bend and to turn the thigh outwards remains during the greater part of the continuance of the complaint. As motion is painful and must aggravate inflammation, quietude of the thigh is evidently very proper in every stage of this complaint. M. David in France, and Mr. Justamond in England, have particularly extolled its utility.

Such are the usual symptoms which denote the formation of a lumbar abscess. I have already noticed the means by which we ought to aim at its dispersion, and I have attempted to shew that the presence of matter is the cause of its accumulation.

Let us now suppose that no efforts on the part of the surgeon could have prevented the formation of an abscess. Let us suppose that the collected matter has descended in the course of the psoas muscle beneath Poupart's ligament, and now presents itself beneath the fascia of the thigh. The symptoms which then characterize the complaint are, the discovery of a fluctuation in the tumour;—the absence of pain in the part, which shews that the fluid was not formed therein;—the intelligence that the patient affords of the pain which he had previously suffered in his loins; and the forcible impulse of matter from the loins into the tumour, which is perceived whenever the patient coughs.

4. Before I proceed to relate the conduct which I should then pursue, I shall endeavour to in-

investigate the cause of that derangement of constitution, which succeeds to the ordinary mode of evacuating the matter from a lumbar abscess; for could this be determined, the most rational means of preventing it would be immediately indicated. This, therefore, is the first point which I shall endeavour to ascertain. The fever, which is excited, appears to be subsequent to the local inflammation, and corresponds, in degree, to the disease existing in the part. It has been generally observed, that where the abscess has spontaneously burst, or has been opened with the least possible degree of irritation, if fortunately but little topical inflammation was produced, the derangement of the constitution was small in proportion,

Our first enquiry will therefore be, to what cause we ought to attribute this local inflammation. Surgeons formerly were accustomed to ascribe it almost entirely to the admitted air, which they supposed to act by powerfully stimulating the cyst of the abscess; and also by producing putrefaction of the contained pus. This putrifying matter was also sup-

posed to act in a twofold manner; first, by irritating and aggravating the inflammation of the contiguous parts; secondly, by being absorbed and conveyed into the circulating vessels, where by its stimulus it occasioned the fever concomitant to the complaint.

These, I believe, are the principal opinions that have been maintained: I wish now to enquire into their truth or fallacy. First, then, is the admitted air capable of so greatly stimulating the cyst of an abscess? and here our enquiry becomes extended: the question may be stated—Does the air admitted into the different cavities of the body cause that inflammation which ensues when they are laid open? or ought we rather to attribute it to the irritation produced by the inflicted wound? Surgeons were formerly inclined to impute very mischievous effects to the entrance of air into cavities: they seem to have imagined it possessed of very deleterious powers. This opinion appears strange, since it is very little stimulating to the animal fibre; and that it does not particularly irritate the membranes of the body, common observation and experimental

mental enquiry have evinced. Air is admitted into the cellular substance in Ephysema, in which, however, it produces no inflammation. Mr. Ashley Cooper permits me to mention the result of experiments which he made, in order to determine how far the air was stimulating.—He inflated the abdomen, thorax, and cellular substance of dogs, and immediately closed the aperture through which the air was impelled; the wounds healed by the first intention, the air was absorbed from the cavities, but no inflammation was excited.

The circumstances, however, are different when the opening is permanent; a constant renewal of air is permitted; and the application of a matter so unusual to these surfaces I am inclined to believe does harm. Whenever the integrity of the cyst is destroyed, though by spontaneous ulceration, or by means productive of the least possible irritation, still much inflammation frequently ensues; for where ulceration of the cyst takes place, little, if any, inflammation is perceptible until the discharge of the pus has hap-

pened :—and when a caustic has been applied to the tunica vaginalis testis, for the cure of the hydrocele, though that membrane has suffered all that it can do from irritation, yet, the severity of the symptoms is always greatly aggravated when the sloughy tunic has ruptured. Whether the unsupported and collapsed state of the cyst is the cause exciting inflammation,—whether this action is occasioned by the sensation of imperfection in the part,—or whether it is owing to the irritation of the admitted air, may be left as matter of opinion ; I am only solicitous to state, that an inflammation appears to me to take place, independent of the local stimulus of the wound.

The next opinion that I have to notice is, whether the admitted air may not do injury by inducing putrefaction of the pus ? If the matter had only an incomplete discharge, it was confined in a state of putrefaction, and thus applied to the surface of the abscess, it surely would be, in some degree, injurious ; but as the outlet in general is sufficient, and as the former matter is washed away by that which is newly secreted, this is not likely to be

be a common occurrence. I here beg leave to remark, that some confusion appears to me to have arisen from the word putrid being used occasionally to signify both foetid matter, and matter in a state of putrefaction:—but putrefaction is owing to chemical decomposition of the animal substance, and matter thus circumstanced cannot fail to irritate the animal fibres.

On what the odour of newly secreted matter depends is not well known, nor does it follow of consequence, that because it is disgusting to our senses, it is stimulating to the surface which secretes it. The foetor, surely, denotes a deviation from its usual properties; and, therefore, probably, it may be capable of irritating the surrounding vessels.—But the degree of irritation we are induced to believe is not great, from observing that ulcers continue for a long time to discharge putrid pus, whilst their surface appears little affected by the qualities of the secretion.

It has been generally remarked, that where there is little irritation consequent to the opening

opening of the abscess, in general, little factor of the discharge is perceived; it appears probable, therefore, that the unnatural qualities of the secretion depend on the local excitement of the vessels.

The third point for enquiry is, whether the absorption of this putrid matter produces hectic fever? Before this question can be discussed it ought to be stated, whether putrifying or foetid matter be meant? We daily see instances of people having copious discharge of foetid matter from ulcers, who do not suffer hectic fever. If matter which is injurious be absorbed, the absorbents are first stimulated, and inflame; and when that matter enters the circulating system, it excites fever by its stimulating properties:—but this is not hectic; it is more violent, more approaching to the nature of inflammatory fever. Now, as there are no symptoms indicating inflamed lymphatics,—as the fever is of a different nature,—and as we have daily instances of large surfaces secreting putrid pus, without the production of fever similar to that attendant on lumbar abscess, I conclude
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that the absorption of matter is not, generally, the cause of the fever which succeeds the ordinary evacuation of the lumbar abscess. To what origin, then, are we to impute this local and general irritation?

There are two causes which seem to induce the topical inflammation. 1st, An irritation extended over the whole cyst from the part wounded or ulcerated.—This is, generally, proportionate by the injury done to the cyst: a knowledge of this circumstance shews the great impropriety of much injuring the cyst, by extensive incision, or the application of caustic. 2dly, Great inflammation sometimes occurs where the local irritation is not considerable. The cause of this I have endeavoured to investigate; and in our treatment of these complaints we should endeavour to avoid those circumstances likely to induce it. There is also a third circumstance very injurious to the health of the patient, which is, a copious and exhausting discharge from an extensive cyst.

The fever appears to be a sympathetic affection

affection of the circulating system with the local discharge. It is more violent during the first stage of local inflammation; and is continued, though moderately, from the exertion and debility attending the production and continuance of the discharge from the cavity of the abscess,

The method which I have pursued appears to me well calculated to obviate all the injurious consequences subsequent to the evacuation of a lumbar abscess. The matter is by this method discharged; and, as the wound made in the cyst immediately unites, inflammation is prevented:—neither does that inflammation ensue, the cause of which, it is difficult to assign, but which appears to me independent of the irritation of the wound inflicted in the cyst. The general circulation also remains undisturbed. It is natural to suppose that the secretion into the abscess would, in some degree, increase the patient's weakness; yet, scarcely any alteration is perceptible. When one of these abscesses held three pints of pus, the loss of fluid from the system was generally in the first fortnight
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twelve ounces; in the second, between six and seven ounces; in each succeeding fortnight, about four ounces. In about six weeks, as no matter is impelled by the disunited fascia when the patient coughs, I conclude that the original abscess of the loins is nearly abolished, in consequence of the disposition of its sides to contract, when they are not kept asunder by collected fluids. And that no repletion of the abscess is likely to happen, evidently appears, for whatever secretion is made into it, will, by its gravity, descend into the cavity left by the separated fascia of the thigh. Thus is the reduction of the abscess, to a very considerable extent, accomplished, without irritation or debilitating exertion of the vascular system. Neither is the loss of fluids which I have stated, to be compared to the profuse quantity drained off from the circulating system, when inflammation is produced and secretion greatly increased, in consequence of the abscess being immediately laid open. I also think it probable that the violent inflammation which is thus induced, thickens the cyst, lessens its elasticity, and prevents that speedy contraction
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which I believe happens when its inflammation is obviated by the means I have related.

A knowledge of the dreadful consequences attending the ordinary mode of discharging the matter made surgeons delay, as much as possible, to open these abscesses. The quantity of pus was therefore suffered to encrease till it had very extensively detached the fascia, and until the dimensions of the cyst in the loins was much enlarged. This practice is evidently injurious; the longer the opening is delayed, the greater does the accumulation of matter become, and the more extensive must be the containing cyst,—in proportion to its extent, so will be the succeeding inflammation and discharge. I have little doubt but if every psoas abscess had been opened on its first appearance fewer people would have died of this complaint. When such an abscess first presents, perhaps, it may contain eight ounces of pus; yet I have seen a case where the fascia was tense and did not readily admit of protusion; in which one-third of it was detached from the subjacent parts, and the abscess contained forty ounces of fluid before the

the tumour became so prominent as to demand of the surgeon to give discharge to its contents. The skin will retain its natural appearance, because it is not distended by the matter,—the fascia sustains all the pressure. If the pus be not evacuated, the quantity collected beneath the fascia must increase. The same observations apply to matter originally formed beneath the fascia, which must be discharged early, if we wish to prevent the increase of disease. The fears of exciting dangerous fever has been the cause of delay; but by the method here recommended the matter may be discharged and the dimensions of the cavity reduced without such apprehensions. After having two or three times punctured the fascia and discharged the matter, no fear I think, in ordinary cases, need be entertained of leaving a permanent opening in the cyst of the abscess, and thus inducing that inflammation which is necessary for its final closure. I scarcely need observe, that where the tumour is small, care is required in the introduction of a lancet beneath the fascia, lest the large vessels be injured, doubtless the
opening

opening should be on the one side, and not opposite to these vessels.—

5. I now wish to represent to the reader the manner in which I propose to treat ordinary cases.—I at first tapped them with an hydrocele trochar; but I found on the redistention of the fascia the pressure against the orifice induced it to inflame and ulcerate. Although this never happened but in one case, as I have related, yet, I was fearful of it in others. In that case, the second discharge of matter was too long delayed, so that the distention of the fascia became very considerable.

I prefer discharging the pus by introducing a lancet through the integuments, then passing it obliquely for a small distance between the skin and fascia, and then by depressing the point of the lancet to puncture the cyst.

When a trochar is introduced, the orifice of the cyst is opposed to that of the integuments; the opening through which the
trochar

trochar has passed is filled up by newly formed vascular substance: when distention takes place, and this substance is pressed upon, we know that it will more readily ulcerate than parts originally formed, and, therefore, there is danger of its giving way from pressure; but when the cyst is punctured in the manner which I have described, the pressure is exerted against the original cyst, and not against a newly formed substance.

If the lancet be introduced too obliquely, the orifice in the integuments will not correspond with that of the cyst, and the discharge of the pus will be difficult, particularly if there be mixed with it flakes of firmer matter. In that case I think a flat trochar should be introduced through the wound. The canula should be of a sufficient size to allow the passage of any flakes of firm substance which may be contained in the matter; for the introduction of a probe through the canula, to remove such obstructions, is obviously injurious; it may stimulate the cyst, and admit air into the cavity of the abscess. The mat-

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ter, if possible, should be drawn off in an uninterrupted current; and the aperture should be immediately, and exactly, closed. It might be said that the canula could be introduced without the stilette; but this, perhaps, cannot be effected without difficulty: every degree of force should be avoided, and the utmost attention paid, that the cyst should not be irritated. I should object much to the introduction of probes or directors through the orifice, in order to expedite the flow of the matter; if the opening made by the point of the lancet be too small, I would rather enlarge it, than risk the production of inflammation of the cyst by such irritation.

The complete evacuation of the abscess seems indispensable in the prosecution of this plan of conduct; for if the matter be but partially discharged, re-distention of the cyst will soon happen, and ulceration of the punctured part will be inevitable, if an outlet be not given to the accumulated fluid. I have usually dressed the orifice made by the lancet with lint, whilst its edges are brought into
close

close contact with sticking plaister, as we commonly treat the wound made in venæ-section. I think it better to make this incision in a longitudinal direction with regard to the thigh; for it appears less likely to be burst open by the exertion of coughing. Much attention to procure the immediate union of these punctures is requisite, as upon this circumstance depends the success of this mode of treatment. I have generally postponed the second discharge of pus for a fortnight; but if the cavity soon fills again, and the newly-healed punctures are irritated by the pressure of the contained fluid, there is an absolute necessity for evacuating the contents of the abscess at an earlier period.

The advantages of the mode of treatment which I have pursued and recommended in these complaints appear to me strikingly evident; yet, I do not expect that in all cases our intentions can be accomplished. The exertion used in coughing will sometimes impel the matter through the puncture, and prevent its immediate union; sometimes inflammation of

the cyst, and ulceration of these orifices, may happen. But if the original plan of treatment has been frustrated, the conduct which has been pursued is, still, the best possible to be practised:—the matter has been evacuated by a small wound, and with the least possible degree of irritation to the cyst of the abscess; consequently, the succeeding inflammation and fever will probably be small in proportion. Since the former part of this Essay was printed, a case has occurred which proves the truth of these opinions.

C A S E.

A man was admitted into the hospital with a large lumbar abscess. I punctured it obliquely with a lancet; but the matter not discharging freely, I introduced a trochar through the orifice, and evacuated thirty ounces of pus. In consequence of violent exertion in coughing, some matter was the succeeding day impelled through the orifice; the puncture was again carefully dressed, in hopes that it might still heal, and for some days the

event was doubtful; but the man, in getting up, had twice displaced the plaister. On the fifth day, an inflammation of the integuments and induration and tenderness of the fascia, were evident: the patient also complained of pain when the tumour was slightly compressed.

The present object of surgery was I thought, to lessen as much as possible the irritation of the punctured parts, that the stimulus imparted to the cyst from that cause might be proportionably diminished. I directed a poultice to be applied to the part, from which he obtained much abatement of pain: on the following day he did not complain, the tumour was, comparatively, little sensible, and soon after became indolent. However, much irritation, both locally and generally, succeeded; the discharge was copious and fœtid; and the fever hectic,—it prevented his sleeping, and induced great debility. After about ten days, the severity of the fever diminished, and his strength began to return. I forbear to detain the reader with a recital of every circum-

stance—it will be sufficient to say, that the severe illness which he has endured, the extreme debility occasioned by the fever and discharge, and the length of time to which his cure will probably be protracted, yet more forcibly impress my mind with the conviction of the advantages derived from this mode of treatment; by which the cure of such an abscess has been accomplished in less time, and without the production of inflammation or evident fever.

I have thus taken the liberty of offering to the public my observations and sentiments respecting these abscesses: I have not interrupted the account by remarking in what they vary from the opinions of others; but I now beg leave, briefly, to point out these differences.

1. The prevailing opinion has been that the matter originated from a disease of the spine. This is directly contrary to the information which I have derived from dissection; and were the idea true, it would surely be

strange, that in nine succeeding cases, to which I was particularly attentive, there was not the least reason to suppose that any such disease existed. That disease of the vertebræ, when it exists, may irritate the adjacent cellular substance, and induce suppuration, is very probable; and, therefore, it cannot be surprising occasionally to find the two diseases concomitant. Where there is the greatest quantity of loose cellular substance these abscesses, which, perhaps, are more the product of constitutional indisposition than local irritation, generally form. Of this nature are the abscesses which take place in the cellular substance surrounding the rectum, and which afterwards contract into fistulous tubes. These cases are, in my opinion, somewhat analogous; yet, in the latter no one ever suspected a caries of the adjacent bones.

2. Perhaps, the idea which I have formed of the nature of the cyst of these abscesses may not accord with the general opinion. The case which I have related, and diffusion of the pus after the evacuation of such an abscess,

which is no uncommon occurrence, are the arguments on which my opinion is founded ; and they appear to me sufficient for its support.

3. The propriety of speedily opening most chronic abscesses, though contrary to general practice, appears to me obviously proper. In phlegmonoid abscesses a spontaneous outlet to the collected matter is speedily afforded by the extension of disease ; but in chronic suppurations this natural opening is very long protracted, and the disease, in consequence, augmented.

Perhaps, this mode of diminishing cavities, by occasionally evacuating their contents, may be applied with advantage to other cases in surgery. In the only case of chronic abscess in which I tried this practice, inflammation ensued in the surrounding cellular substance, when the matter became diffused : but I do not think it is likely to happen in other cases.

As the design of this treatment is merely to diminish

diminish the extent of an abscess, I think it will not be found applicable to collections of matter beneath the fascia of the thigh; for the extent of the detached fascia cannot be much diminished. I should propose in such cases early to discharge the collected matter, by means as little irritating as possible to the surrounding parts: for the effect of much inflammation is a profuse discharge, which often greatly exhausts the constitution. Having thus avoided immediate inflammation, we should next endeavour to procure an union of the separated parts, and to effect such a design I think, together with the use of bandages, we are then vindicated in employing stimulating applications.

I submit to the reader whether a mode of treatment similar to that which I have related may not be sometimes proper in cases of spina bifida. The reason of the accumulation of fluid in these diseases beneath the Dura Mater is not very apparent; nor does the cause producing the secretion appear to be powerful or constant; for the water collects very slowly at first, and in some cases none has
ever

ever been effused, and the child has grown up without experiencing any inconvenience. When once the collection has begun, the cause of its continuance and increase is evident; the collected fluid irritates and distends the membrane which secretes it, and thus augments the disease. I do not know that any attempts have been made to remedy these complaints; but I see no reason why we should forego all endeavours. I think it very probable that a gentle degree of pressure made on the tumour from birth, or at its commencement, might produce the absorption of any deposited fluid, and thus prevent the distention of the unsupported Dura Mater.

It has been an opinion, too generally adopted and inculcated, that the imperfect formation of a part so essential to the animal, implies a deficiency of power in the constitution. This reasoning appears to me fallacious. Want of vigour of constitution might cause debility in any part, but could not cause an error of formation. I have seen very healthy infants who have been thus imperfectly formed, and
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whose health has sustained but little derangement till the tumour has burst, when they have perished from the inflammation of the Medulla Spinalis which inevitably ensues.

Surely, some attempt ought to be made to repress this collection of fluid: but even if it should have begun, I would still think it right to endeavour to produce its dispersion, by the application of gentle pressure. If this should have no effect, and should the accumulation of fluid continue to increase, as the death of the patient would be inevitable on the spontaneous rupture, I think it would be vindicable to discharge the fluid by a puncture with a finely cutting instrument, and endeavour to heal the wound immediately; and should this be accomplished, to attempt to repress a future collection, by bandage, or by those topical applications which appear best adapted to this purpose.

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is, however, some attempt ought to be made to
repel this collection of blood, and even if it
should have begun, I would still think it
right to endeavor to prevent its diffusion,
by the application of gentle pressure. It might
be thought, however, that the rupture of the
arteries of the spinal cord is inevitable on
the spontaneous rupture. I think it would be
vindicable to disengage the cord by a puncture
with a finely cutting instrument, and endeavor
to heal the wound immediately; and
should this be accomplished, no attempt to
repel a future collection by bandages, or by
other topical applications, which appear best
adapted to this purpose. I would give them all
up, and say, that the rupture of the
arteries of the spinal cord is inevitable, and
that the rupture of the spinal cord is inevitable.

ESSAY

ON THE

COMPOSITION AND ANALYSIS

ANIMAL MATTER.

MR. Boyle hath shewn by experiment, that vegetables will grow, and will produce a woody vegetable substance when in contact only with air and water: now, as pure water is a compound of two airs, the solid fabric of vegetables must be a modification of the same particles of matter, which might previously have existed in an aeriform state.

Some observations have made it appear probable, that even animal matter may, under certain circumstances, be formed of similar

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ingredients. The stationary life and growth of marine zoophytes induce us to this opinion, their sustenance appearing chiefly, if not entirely, to be derived from the surrounding air and water. At certain seasons of the year, immense shoals of fish transport themselves from one part of the ocean to another; the foremost would surely devour all the food which they might accidentally meet with; yet those which follow do not seem to suffer from deficiency. However, it may be remarked that animals of this nature do not need that constant renovation of substance which those of warmer blood require: yet still the long abstinence they undergo, without adequate diminution of bulk, appears to confirm the opinion.

Dr. Fordyce, in his Treatise on Digestion, says, that he has put this idea to the test of experiment: he kept gold fish for six months in distilled water, during which time they grew.

Believing that, if this supposition of the formation of animal matter could be proved
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to be fact, it would render our knowledge of its composition and analysis clear and simple; and foreseeing that some curious deductions in chemistry would follow its establishment, I sought to ascertain or refute the opinion by the following experiments. I first resolved to repeat, in different ways, the experiments of Doctor Fordyce; yet I soon perceived that experiments thus conducted could not be conclusive. I shall only present the reader with a general sketch of what was done on this plan, as it were unnecessary and improper to relate with accuracy every circumstance of indecisive experiments.

EXPERIMENT.

I placed some frogs spawn in a large earthen pan, with plenty of clear river water; and when the tadpoles had extricated themselves from the viscous matter of their ova, I removed them into distilled water; they were about two hundred in number.

At first the distilled water was renewed once a week, but afterwards it was merely filtered:
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the pan was placed in a garden and covered with moderately fine linen; however, any covering, which did not exclude the air, was insufficient to prevent the admittance of the ovulae of minute vegetables and animals; each week, when I filtered the water, I separated from it abundance of dark-green vegetable matter, and great numbers of animalcules were left struggling on the filter. The presence of these were not so defeative of the intention of the experiment, as at first sight, might be imagined; for, excepting only the imperceptibly small quantity of matter contained in the ova, these insects and vegetables grew from pure air and water alone; so that admitting the tadpoles fed upon these, they still were supported by a modification of air and water. Many of the tadpoles died, and the others, I believe, fed on them: those that died were however separated from the others, weekly, by filtration.

I observed that the tadpoles were not, as in common, restlessly seeking for food, but lay quietly at the bottom of the vessel, unless disturbed,

disturbed; yet, when the vessel was shaken, they swam with much vivacity. This experiment began on the 19th of April, and on the 21st of June I counted forty tadpoles, many of which were in a state of transformation, and four perfect frogs. Their number now quickly diminished, for, after having undergone this transformation, they either died or escaped from the pan; had they died their remains would have been visible, yet none were discovered. I did not suppose they could have climbed up three inches height of a glazed pan, yet this they doubtless accomplished.

EXPERIMENTS.

The following year I took twelve leeches, which weighed ten scruples, and put them into two gallons of distilled water, in a glass jar. I covered the jar with two layers of paper, and pricked holes in the cover with a pin, to give admission to air. I never filtered this water, but suffered the leeches to remain in the same state for three months. Much vegetable matter had grown in the jar during

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that time. When I then examined them, eight only were living, but these weighed twelve scruples; so that the weight of the leeches had encreased, although their number was diminished.

I also caught some small tadpoles, which weighed two drams and one scruple. I put them in a similar bottle and covered them in the same manner as the leeches; they lived, but did not appear to grow; at the end of a month three were found dead, and the remainder weighed one dram and one scruple. These experiments were made in London.

The impossibility of excluding the ovulæ of animals and vegetables from water, must, I think, always make these kind of experiments indecisive. The conclusion which I have drawn in my own mind, from these and from future experiments, is, that the less perfect animalcules are capable, like vegetables, of converting mere air and water into their own nature, and that animals of a still higher order may derive occasional sustenance from the same materials;

ANIMAL MATTER.

materials; yet, as nature probably designed them to be supported by already prepared animal and vegetable matter, the sustenance which they derive from mere air and water is imperfect, and inadequate to their continued support.

The experiments which I made upon this plan were all executed during the summer season, the prevention of vegetation was then impossible; for, when I attempted to obviate this circumstance by closing the bottle, even the leech, the most vivacious of these animalcules, quickly perished. In the winter, perhaps, vegetation might cease; but then, the torpid and little varying state of the animal body would, I think, render the experiment indecisive.

It has been long well known, that vegetables will grow when in contact only with pure water and air. I do not, however, know that their analysis has been attended to. Mr. Boyle indeed says, that they yielded a spirit and a caput mortuum; but the extent of chemical
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knowledge in his time, did not enable him to investigate this subject, as he would have wished. I was desirous to know if vegetables thus procured would yield the same substances as are afforded by the decomposition of common vegetables; for if from these we can obtain vegetable alkali, lime, iron, and charcoal, it follows, that these substances are only varieties of arrangement of the same particles of matter, which previously existed in the state of air and water. To ascertain this circumstance, I made the following experiments.

EXPERIMENT.

I strewed the seeds of cabbages on thin clean flannel, spread on glazed earthen plates, and each day sprinkled them with distilled water; the seeds soon began to vegetate, and the young plants grew as speedily and vigorously as usual. After five weeks I mowed off the young plants from the seeds and roots. I took nearly two ounces of these young plants and reduced them to ashes in a crucible; they yielded eight grains of whitish ashes; this was a smaller proportion of fixed residue than what I
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obtained from common vegetables, for six drams of common cabbage-leaf yielded when burned, five grains of ashes. To the eight grains of ashes half an ounce of distilled water was added, and gently heated; upon dipping some paper, tinged with the vegetable blue into this liquor, it instantly became of a bright green. The water being poured off, one dram of marine acid diluted was added, which produced a gradual long-continued effervescence; this being filtered was divided into two equal parts, to one portion the prussicated alkali was added, and a very copious precipitate of prussian blue was produced; the other portion was supersaturated with caustic vegetable alkali, and a very plentiful flocculent precipitate of lime was caused: still some fixed residue remained, which doubtless was charcoal.

To this experiment it may be objected, that the vegetables derived the substances which were discovered on their analysis, from their seeds, or from the flannel on which they grew. A quantity of seed equal to that which

produced these plants, being burned in the same fire, left too small a quantity of ashes for examination: neither is it likely that the flannel should be decomposed by the growth of vegetables: however, should any experiment of this kind be in future attempted, I think it would be better to grow the vegetables, if it be possible, on sand, which had been previously well washed with marine acid, by which it would be freed from accidental admixtures.

EXPERIMENT.

I took six very small sprigs of mint, which weighed thirty grains, and put them in phials filled with distilled water in a hot-house. Roots shot from them in plenty, but the stem and leaves grew very scantily: it was in the autumn when probably their power of vegetation was diminished. When the roots had filled the bottles I removed them and burned them in a crucible and obtained eight grains of ashes. Thirty grains of similar sprigs of mint had been previously consumed, and had left too small a residue to admit of examination. To the

the eight grains of ashes thus obtained, some distilled water was added, and gently heated. This liquor instantly changed the vegetable blue to a deep green colour; thirty drops of marine acid, diluted with distilled water, was now added, which being filtered, was divided into two equal portions: one part being saturated with caustic vegetable alkali, a flocculent precipitate of lime was deposited; and the prussicated alkali, being added to the other, a considerable quantity of prussian blue was produced. Some ashes still remained undissolved by the marine acid: these I conclude are charcoal.

This experiment is not liable to the same objections as the preceding one, here the vegetable matter could only be produced from air and water, yet, when analysed, it yielded the same products as other vegetables. It may be questioned if the marine acid which I used was pure, and the tests which I employed were good; it is therefore right to mention, that I mixed the same proportions of marine acid and prussicated alkali as were used in the experiment,

ment, and though a slight blue colour was exhibited, the appearance was very different to that copious blue precipitate which was thrown down from the acid which had stood in the vegetable ashes. Neither was any precipitate caused when the marine acid was supersaturated by the vegetable alkali.

Every one knows how readily mint grows in common water, and few people, I believe, will doubt, that this mint when analysed, will yield vegetable alkali, lime, iron, and charcoal; yet, surely the water does not contain these substances in such quantities as to impart them in a ready-formed state to the vegetable. Some authors have questioned whether the alkali formally exists in vegetables, or whether it is produced by their decomposition; to investigate this subject I made the next related experiments.

I took six drams of cabbage-leaf, and added one ounce of strong marine acid; after it had stood two days I strained off the acid and burned the remaining leaf, and obtained four
grains

grains and a quarter of whitish ashes. To these three drams of distilled water was added, the vegetable blue was not changed by this liquor; the water became, however, salt to the taste, and, on evaporation, left about half a grain of saline crystals; twenty drops of marine acid diluted was added to the remaining ashes, half of which being saturated with caustic vegetable alkali, an evident precipitate of lime followed: and ten drops of prussicated alkali being added to the remainder, a deep blue was produced: still some charry matter remained undissolved by the acid.

Equal quantities of strong marine and nitrous acids were suffered to stand some days on fresh parsley, but they took up neither lime, iron, nor alkali; some of the same parsley reduced to ashes by heat yielded abundance of these materials. There is surely nothing in the mild juices, and delicate texture of vegetables, which could protect these substances if already formed from the attraction of such potent menstrua. If these substances are formed by the decomposition of vegetable matter,

matter, it is clear, that they are not elementary or unchanging, but only varieties of arrangement and motion of the same atoms, which differently combined formed a vegetable, and which might previously have existed in the state of air, or water.

EXPERIMENT.

I was desirous to know if an animal would live and grow when fed only with vegetables, which had been produced from air and water. I therefore procured two rabbits, six weeks old, the produce of the same mother, one weighed twenty-three ounces and a half; this I killed and analysed: the other weighed twenty-three ounces, which I fed in the following manner. Having obtained a large quantity of young cabbages and lettuces, which grew on flannel and were only sprinkled with distilled water, in the manner before related; I mowed off the tops and gave them for food to the rabbit. On the third day after he had fed on them, he appeared very ill; he breathed very quick, and his hair was ruffled; he was also purged: fearing that he would die, I gave him

him a few shelled oats, still leaving a plate with these vegetables before him. Next day he appeared much better and had eat both the oats and greens. For four succeeding days he eat a plateful of the fresh vegetables, and a small quantity of oats, he appeared thin but was very lively: he only eat two ounces and a half of oats in the week, a quantity, I think, very insufficient to support him in the state he appeared to be, had not the vegetables contributed to his nourishment. I was now obliged to go into the country for a few days, the rabbit was therefore fed for a week with common cabbage-leaf and parsley. After this I fed him for a fortnight in the former manner, allowing him scarcely any oats; he eat as much cabbage as grew upon six plates each day; he was lively and strong, and though he looked thin, yet he encreased two ounces in weight during this fortnight. Deficiency of vegetables obliged me to discontinue the experiment: I did not analyze the rabbit, as I had originally intended, because I thought the experiment incomplete, and I designed to repeat it more extensively the following year. I did

did procure every thing for that purpose, but the garden where I attempted to grow these vegetables was too near to the smoke of London, which prevented their vegetation.

If the vegetables which are thus produced contain the same matter as common vegetables, though in a less degree, there appears no reason why an animal should not be supported by them. Let it be then remembered that such an animal is nourished, only by a modification of air and water, from which his organs are capable of forming the mineral alkali, phosphorus, and every substance which formally exists in the animal body. I have no doubt but that an animal can be thus nourished; caution is, however, required in the experiment; for it is probable the vegetables will at first disagree with his stomach. Some other food should then be allowed him, and if the quantity and analysis of that food be estimated, the object of the experiment will not be defeated. If a young rabbit, living on these vegetables, encreases one pound in weight, and during the time of this growth
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eats only one pound of oats; if the oats on analysis will yield only a certain quantity of lime, iron, and charcoal, and the animal, when analysed, yields a larger proportion of lime, and iron, together with mineral alkali, and phosphorus, than what is produced from this food, or than could have been obtained from that animal before the commencement of this experiment; it is then sufficiently evident, that the animal body has a power of preparing these substances, or in forming an animal matter, which yields them on its decomposition. Whoever also reflects on an animal being perfectly supported by vegetables, and remembers that his circulating fluids contain abundance of mineral alkali, which the art of chemistry cannot extract from the vegetable; must perceive, that the animal powers are certainly capable of forming this substance.

The chicken contained in an egg, after incubation, has lime deposited in its bones, has red particles and consequently much iron in its blood, and probably a proportionate quantity of mineral alkali. I wished to know if these substances

substances existed in similar quantity, in the egg before incubation, and to discover this circumstance made many experiments, the results of which were not exactly similar. I should needlessly engross the time of the reader if I were to detail separately the circumstances of each experiment: it will be sufficient that I relate the general results. I ought in this place to mention, that the eggs, and chickens, on which these experiments were made, were all produced by the same hens. A chicken contained in the egg shell, after complete incubation, weighs in general six drams less than the contents of the shell before that process. There is, therefore, more matter emitted, than imbibed, through the shell during incubation.

I reduced an equal number of chickens and eggs to ashes, sometimes in retorts, sometimes in crucibles. On the ashes I digested first some of distilled water and obtained the salt contained in them. In some experiments the quantity of salt found in the ashes of chickens greatly exceeded that found in the

ashes of the eggs; in others it still exceeded, but in a less proportion, and in other experiments the quantities were equal. I next digested marine acid a little diluted on the ashes, which took up the lime and iron contained in them. I precipitated the lime by saturating the acid with the vegetable alkali.

That the precipitate was lime, was proved by pouring on it diluted vitriolic acid, with which it often formed a stone like selenite. The average proportion of lime to a chicken was five grains; but the egg yielded in general scarcely one grain.

On adding to another portion of this marine acid the prussicated alkali a copious blue precipitate ensued, and in quantity so nearly equal from the eggs, and from the chickens, that I could not decide which had the superiority. After this I boiled vitriolic acid on the ashes, expecting by this means to take up any lime which might be combined with phosphoric acid. Upon saturating the vitriolic acid with vegetable alkali a cloud like precipitate

pitae appeared, and, I think, generally in equal quantities, whether it was obtained from the egg or the chicken; but the precipitate was too small in quantity to be weighed. I afterwards heated the remaining ashes with charcoal. Yet still there was a good deal of fixed residue, such I believe has ever been found to remain after chemists have prosecuted to the utmost, the analysis of animal matter.

I also investigated the difference of the substances found in the egg and chicken, by digesting on them immediately strong marine acid; this would certainly take up whatever alkali, lime, or iron existed formally in those bodies. Marine acid also combines with vegetable and animal gluten. The precipitates obtained by saturating the acid were, therefore, not pure and might deceive. I found, however, an excess of lime in the chicken, when thus examined, proportionate to that which had appeared in the former experiments; and also a larger quantity of salt was obtained, but I could not decide which body possessed the superior proportion of iron. After I had filtered

tered the marine acid from the eggs and chickens, on which it had been digested, and washed the residues with distilled water, I consumed them in crucibles, and still obtained from the ashes which remained, more lime, iron, and salt.

This circumstance proves to me, that the substances found in the ashes of burned animal matter, do not all formally exist in the mass before its destruction; but are only new combinations of the same ultimate particles, which under their former mode of arrangement made the animal substance, but which being driven asunder by the repulsive power of fire, are left at liberty to form other modifications of matter.

It is, however, right to mention, that the same results did not always follow experiments similarly conducted; for after having macerated some lean beef in water, till it had lost all its colour, I digested on it a large quantity of strong marine and nitrous acids. I then washed and consumed the residue, but could

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not from its ashes obtain an evident quantity of lime, iron, or salt. This circumstance I account for by observing, that these acids so completely dissolve the animal gluten, that the residue is no longer capable of producing such compounds. Yet the truth of the preceding observation remains established; for if the acids be saturated by an alkali, they will precipitate that matter which they had taken from the animal substance, and it will be found merely gluten, containing neither lime, iron, nor alkali.

Lime, it is evident, exists formally in the chicken in much greater quantities than in the yolk or white of an egg; and to me it appears that it is formed in the process of animalization of the chick. Yet it may be contended, on the contrary, that it may be taken from the egg-shell. In answer to this, I can only say, that the inner surface of the shell appears smooth after incubation, and not as if any matter had been taken from it. I have also taken the shells of eggs, when the time of incubation was nearly expired, and compared them

them with shells before incubation, dissolved their lime in marine acid, and afterwards obtained an equal weight of precipitate from each.

If the ultimate particles of animal matter be the same, and if the various products obtained from it, depend on the accidental combination of those particles : it accounts for the want of uniformity which is observed in the quantity of the substances procured by its decomposition. It also seems probable, that these products will differ as the mode of destroying the original matter varies. It occurred to me, that there would be less fixed residue, when the particles were suddenly driven asunder by the repulsive power of fire, than when they were gradually separated by spontaneous putrefaction : in which process, they would for some time remain within the sphere of each other's attraction.

EXPERIMENT.

It being requisite to bleed two persons, who had suffered accidental injury, but

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who in other respects were healthy ; I caught in a common crucible five ounces of blood from one of them, and four ounces from the other ; an equal quantity of blood from the same people was collected in another crucible. The blood contained in the one vessel was immediately decomposed in a moderately strong fire, and yielded five scruples and two grains of ashes,

The other crucible was carefully covered, and hung up in the corner of a chimney ; where it was exposed to the varying heat of the fire for four months. Whilst it remained in this situation, I added to it, four different times, three ounces of distilled water ; for had it been suffered to dry, the process of putrefaction would have ceased. At the expiration of four months I put the crucible in the fire, and expelled all the volatile matter from its contents ; the remaining ashes weighed only seventy-eight grains. The ashes which I had obtained from the immediately decomposed blood, I had put by in a paper, in a covered gallypot ; the salts contained in them had deliquesced. I now added distilled water to both parcels of
ashes

ashes, and having thus dissolved the salt contained in them, I evaporated the water, and obtained from the first ashes only six grains of salt; from the latter (the residue of the putrified blood) I procured fifteen grains. Strong marine acid was now digested on each parcel of ashes, which being examined by saturation with the vegetable alkali, and by the addition of the prussicated alkali, it was found that those ashes left by the putrefaction of blood, contained near forty grains of aerated lime; whilst in the other residue, there was not half that quantity: the former ashes also contained more than twice the quantity of iron, than was found in the latter.

Vitriolic acid was now boiled on the ashes, which took up from each nearly an equal quantity of lime. The residue of the blood, which was immediately reduced to ashes, weighed but a few grains, whilst the remains of that which was suffered to putrify weighed thirty-five grains. The ashes were now mixed with charcoal and put into a strong fire, in which situation they lost much of the weight; being
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now again examined, it was found that they contained nothing diffoluble in strong acids.

In prosecuting the analysis of animal matter, the nature of the remaining refractory ashes naturally excite attention; it might be suspected that they might contain lime and iron, so combined as to be insoluble in chemical menstrea. This supposition is not probable, for when they are mixed with charcoal, they may be almost entirely dissipated in a strong fire.

These experiments, I think, shew that vegetables and animals possess organs capable of assimilating to their own nature, the matter destined to their nutrition; that the animal powers can prepare salt, lime, and iron, which are found to exist formally in the body; however, the greater part of an animal, or vegetable, is without such substances, yet, when destroyed by fire, its component parts do in general recombine, and thus produce these substances. Since then, animal matter is only a peculiar arrangement of common matter, why

why may not the organs of imperfect animals be capable as well as vegetables, of producing this arrangement? it seems probable that they are capable; but animals of more perfect organization, who possess sensation, and were designed by nature to live on previously prepared animal and vegetable matter; they appear less able to accomplish such conversion, and die ere it is effected, from the derangement of their sensitive organs.

When the substances obtained by the analysis of animal and vegetable matter are once formed, their particles mutually attract each other with such force, that fire, or any artifice employed by the chemist will not decompose them. Yet, though their composition cannot be shewn by analysis, these experiments prove it by synthesis.

I was prompted to undertake these experiments, because it was necessary to give some account of the nature of animal matter, in a course of anatomical lectures, previously to describing its arrangement, in the structure of
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the human body : and also, because I had imbibed the opinions of the great philosophers of this island, who, from reflection and reasoning, were induced to believe, that the ultimate particles of matter were the same, and that the various substances, with which, this world presents us, were only differences in the arrangement and motion of similar particles. The testimony of experiment, appears to me to be now added to the truth of an opinion, formerly supported merely by the suggestions of reason. Whoever also reflects on the wonderful divisibility of matter, will scarcely suppose any essential difference in its ultimate parts.

These experiments, it is true, only extend to those substances found in animals and vegetables ; as the vegetable and mineral alkalies, phosphorus, lime, iron, &c. but if lime and iron can be thus composed, why may not clay and gold ?

The reader will perceive that the train of experiments, which I have related, are similar
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to those by which Mr. Boyle supported the same opinion. But the extent of chemical knowledge, in his time, did not enable him to examine the nature of the ashes, left after the combustion of animal matter : he could therefore only suggest a probable opinion, he could not offer to the incredulous, convincing proofs.

I much wish that these experiments were more diversified and extended : I undertook them only for my own information, and having prosecuted them so far as to convince myself, I desisted from further investigation. An opportunity now presenting, I offer them to the public notice ; because, to me it appears, that the late advancement of chemistry, though it has given us great knowledge of the properties of every species of matter, yet has tended to contract our views ; it has made us direct them to partiular objects, and cease to contemplate this beautiful and extensive prospect of matter, and its combinations. The best chemists, M. Lavoisier, M. Chaptall, and M. Fourcroy, either entirely avoid the consideration of the elements of matter, or if they do

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speak

if they speak of them, they do not seem to advert to, or understand, this beautiful theory.

If the related experiments should be considered insufficient to prove these doctrines, I heartily hope it may excite others to further investigation: so that, if the opinion be true, it may be perfectly ascertained. For I know not any thought, which, on contemplation, can so delight the mind with admiration of the simplicity and power evident in the operations of the Creator, as the consideration, that by different arrangement and motion of similar atoms, he has produced that variety of substances which are found in the world, and which are so conducive to the wants and gratification of the creatures which inhabit it.

ERRATA.

Page 25, line 12, *after* there *add* was.

Page 40, line last but one, *for* my *read* any.

Page 47, line 20, *after* surrounding *add* parts.

Page 48, last line but three, *for* late *read* lately.

Page 56, line 15, *for* stimulas *read* stimulus.

... of ... and ...

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AN
E S S A Y
ON THE
NATURE OF THE MATTER PERSPIRED
AND ABSORBED FROM THE SKIN.

DOCTOR Priestley after having well explained the effects, reciprocally produced on the air and the blood, by the process of respiration; proceeds to investigate those alterations of the atmosphere, which, animal perspiration causes. He however only slightly pursued this subject, nor is it a wonder, that he failed to discover that information, which he did not attentively seek. He has asserted that animal perspiration does not injure the purity of the air, in the man-

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ner effected by the process of respiration. But the experiments here related, clearly contradict this assertion, and establish a similarity in the nature of the matter exhaled from the lungs, and the skin of the human body.

Dr. Ingenhouze observed, that air, was perspired from the bodies of animals, as well as of vegetables, but as this was not the immediate object of his enquiry, he did not very attentively examine its nature. He says, "it seemed to be partly fixed air, as it was somewhat absorbed by water;" and the remainder was far from being good respirable air.

Mr. Cruikshank entertained the opinion, that the matter of perspiration, and that expelled from the lungs in breathing, were similar in their qualities. In his experiments, he collected the aqueous exhalation from the skin, but only slightly examined the aeriform matter: he agitated lime water in the air with which his hand had been surrounded,
when

when the precipitation of the lime shewed the existence of fixed air; he has also observed that a candle burned dimly in this air. Such I believe was the extent of the information, which we heretofore possessed on this subject.

In the summer of 1791 I had made many experiments to ascertain the nature of the matter, perspired and absorbed from the skin: but the winters cold obliged me to desist, before I had compleated my design. In the spring of 1792, the Reviewers announced to the public, that M. Lavoisier and M. Seguin had delivered a paper on this subject, to the royal Academy of Sciences at Paris. Much time may elapse before the contents of that paper will be made public, and even then, it may not meet with common perusal in this country.—As I know not the nature or extent of these experiments, and as those which I have made, appear to me satisfactory, and meriting attention; I have therefore resolved to submit them to public notice.

royal Academy of Sciences at Paris. March

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trough. I measured the surface of my hand, and wrist, as accurately as I could with paper, and computed their extent to be seventy square inches. These experiments will probably afford but an imperfect estimate of the quantity of the matter of perspiration; for the propensity of the hand to rise in quicksilver, occasioned a considerable pressure of the wrist against the edge of the bottle, which often impeded the circulation, and benumbed the sensibility of the hand, and probably interrupted the functions of the skin. The coldness and weight of the quicksilver would also tend to impede perspiration.

It is right to mention, that more experiments were performed than are related: I selected those, the events of which, were most common, and which appeared to have been made with most accuracy.

Experiment A. Thermometer about 60°.

I filled a glass jar with quicksilver, which being inverted, and confined in the way I have

described: I held my hand ten minutes in the trough, beneath the surface of the quicksilver, and frequently moved it in that situation, in order to detach any atmospheric air which might accidentally adhere to it, and afterwards introduced it into the inverted jar. The quicksilver soon acquired a degree of warmth, which rendered it not unpleasant; minute air bubbles ascended to the top of the quicksilver, more speedily in the beginning of the experiment, more tardily towards the conclusion. After an hour had elapsed, I withdrew my hand; the bubble of air which now appeared on the top of the quicksilver, was, I suppose in bulk, equal to one scruple of water. In sixteen hours I collected an half ounce measure of air, which makes fifteen grains the averaged product of an hour. No kind of moisture appeared on the surface of the quicksilver, some sucking paper was put up, which was withdrawn unmoistened. My hand was always damp when taken out of the quicksilver: whatever aqueous perspiration was produced, adhered to its surface, whilst the aeriform, from its levity, ascended to the top of
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of the mercury. To the air, which I had thus collected. I threw up lime water, when about two thirds of it were rapidly absorbed: to the remainder I added a bubble of nitrous gas, but could discern neither any red fume nor diminution of quantity. I repeated this experiment six times, with familiar, tho' not uniform events. I believe it will be found that the air perspired, consists of carbonic gas, or fixed air, a little more than two thirds—of nitrogenous or phlogisticated gas, a little less than one third. In one experiment, the nitrogene made only a fourth part of the air collected, and in another I thought it exceeded its usual proportion of one third.

I believe it will not be doubted, that the air which remains after the separation of the carbonic gas, is entirely nitrogenous. It would be very strange, should atmospherical air, which is a compound, be perspired. If nitrogene be superabundant, and injurious to the body, and therefore thrown out from its surface; it is not probable, that oxygene which is salutary, and requisite to the animal, should in the same

manner be dissipated. On the repetition of these experiments, much variety in the quantity of air perspired, was remarked. At one time the quantity collected in nine hours, occupied the space of only thirty-two grains of distilled water: at another time, in three hours, it equalled the bulk of two scruples of water.

As I concluded, that perspiration was obstructed by the weight of the quicksilver, and also by the pressure of the hand, against the edge of the bottle; I thought this process would go on more naturally, beneath water: I supposed if I made an allowance of two-thirds for the quantity of carbonic gas absorbed by the water, I should thus be able more nearly to compute the usual quantity of air emitted from a certain extent of the skin. In this conjecture, the event proved that I was mistaken.

Experiment B. Thermometer 6c°.

Having filled, and inverted a jar in water, I held my hand and forearm for ten minutes
beneath

beneath the surface of the water, frequently moving it, to detach any adhering air, and then introduced it into the inverted jar. The surface of the skin, from whence air could arise into the bottle, was as nearly as I could compute, one hundred and twelve square inches. My arm soon exhibited a curious appearance; it was magnified by the convexity of the glass, and every pore seemed covered by a little spherule of air, which on agitating the water was detached, and quickly ascended to the top. After an hour had elapsed, a bubble of air was collected, which as nearly as I could guess equalled in bulk half a dram of water. In the prosecution of this experiment great variations in the quantity of perspiration were remarked. I once held my hand an hour in the water without any apparent addition being made to the quantity of air collected. When I had thus, in nine hours obtained about three drams of air, I discontinued the experiment. The air remained unattended to, nearly a week, when the quantity was diminished to about one dram and

and a half, which being examined by nitrous gas, was found to be entirely nitrogenous.

I repeated this experiment, and each day changed the water in the trough, when a much greater quantity of air was collected. I procured, on an average half a dram each day, which was entirely nitrogenous. I attempted a similar experiment, in moderately warm water, but procured scarcely any air; the small quantity which was collected, was quickly absorbed by the water.

I heated myself by exercise, till the aqueous perspiration was moderately copious, but at that time less air was procured.

The conclusions which I have drawn in my own mind, from these and other experiments are, 1st, That the remainder of the perspired air, after the separation of the carbonic gas, is nitrogene. 2dly, That the quantity cannot be well estimated, because the water absorbs some portion of it. Water deprived of its air by boiling, evidently did so in one experiment,

riment, and whenever the water remained unchanged less air was produced. I think it probable that the skin absorbs the air dissolved in the water, and thus the disposition of the water to imbibe air is augmented. 3dly, I have observed that, when by exercise, aqueous perspiration was increased, less air was then produced; if the same vessels secrete both these fluids, this observation would naturally be expected. When the circulation is moderately carried on, insensible or aeriform perspiration is chiefly continued; but when the determination of blood to the surface is rapid and powerful, water is poured forth from the exhalents, and the perspiration becomes sensible.

The related experiments clearly prove the quality of the *matter exhaled from the skin*, but they do not well show the quantity; it was necessary to prosecute them further, and I could only depend on the result of those, which were conducted beneath quicksilver. I could however prevent the surface of my hand sustaining the pressure of the quicksilver, by filling the jar into which it was introduced, with
air.

air. The examination of this air would inform me, not only of what was added to it, but also of what was taken from it by the skin, in the same space of time. I therefore exposed my hand in succession to different kinds of air, beginning with that of the atmosphere. It is right to remark, that these experiments do not admit of great accuracy. I estimated the relative quantity of airs, found on examination, by marking on the bottle, the elevation and descent of the quicksilver, and afterwards filled the glass with water to these marks, which being weighed, afforded me the information that I wanted.

Experiment C. Thermometer between 50° and 60°.

I filled and inverted a jar in quicksilver, and threw up into it, one measure of atmospheric air, which could contain seven ounces of water. The quicksilver was depressed two inches and a half from the top of the jar. After moving my hand ten minutes beneath the surface of the quicksilver, to detach any common air which might adhere to it, I put
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it up into the air in the jar, and there retained it for the space of an hour. Before I withdrew my hand, I depressed it beneath the surface of the quicksilver, still keeping it within the glass, and agitated it in this situation, for ten minutes; this was done that I might not remove any of the air, which was the subject of the experiment. The same conduct was pursued in all the subsequent experiments. After five hours exposure of the hand to this air, the quantity in the glass was diminished about half an ounce. It might have been expected that the perspiration would have increased the bulk of the air, but in this experiment, the absorption seemed to surpass in quantity the secretion.

I now threw up into the jar, lime water, by which nearly an ounce of air was rapidly absorbed, and the lime was precipitated; the remaining air being examined by the addition of nitrous gas, was found to contain nearly one-sixth less of oxygenous gas, than it did before the experiment.

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In another similar experiment, after the hand had continued nine hours in the air, I found more than one ounce measure of carbonic gas had been produced, and the remaining air being examined in the eudiometer, contained one-fourth less of oxygen than before the experiment.

The quantity of carbonic gas produced in these experiments, much exceeds that, exhaled from the hand when surrounded by quicksilver; in that situation, I thought half a dram in an hour, a copious product; but in air more than twice that quantity was constantly obtained.

The whole of my hand was not in these experiments surrounded by air; some part of it was still pressed upon by the quicksilver.

It might be enquired, does the oxygenous gas of the atmosphere contribute to the formation of the carbonic gas? Both reason and experiment reply that it does not, for if
oxygenous

oxygenous gas combined with carbone on the surface of the skin, much heat should be produced at the time of their combination; but this production of heat is not found to take place.

Experiment also shows that carbonic gas is perspired from the vessels, for into whatever air the hand be immersed, the quantity of carbonic gas given out, will be nearly the same. This is the first point which I wish by experiment to establish.

Experiment D. Thermometer between 60° and 70°.

Having filled and inverted a jar in quicksilver, I put up into it a seven ounce measure of nitrogenous gas. I pursued the plan related in the former experiment, to avoid adding to, or abstracting from, this air. After two hours exposure of the hand, I thought the bulk of the air had increased, but after five hours had elapsed, I could perceive no difference in the quantity. On throwing up lime water, a rapid and considerable diminution

tion of air followed ; rather more than an ounce of carbonic gas was produced, when no oxygen was present. The increase of the quantity of carbonic gas is accounted for, by the heat of the atmosphere being greater, which disposed the skin to more copious perspiration. Nitrous gas had no effect on the remaining air.

I made similar experiments with the hydrogenous and nitrous gases ; in these an equal quantity of carbonic gas was produced, and when the hand was surrounded by oxygen, the quantity of carbonic gas was not greater.

The reader will perceive that in all these experiments much absorption had taken place, the quantity of air contained in the jar was scarcely perceptibly increased in any experiment, and when atmospherical air or oxygenous gas was employed, it was generally much diminished. I forbear however particularly to remark on this circumstance, as I think the quantity absorbed will be much better determined by other experiments.

The

The whole of the hand was not in contact with the enclosed air, and though I endeavoured to prevent it, some addition or abstraction, might be made in the introduction, or removal of my hand.

I next wished to discover what effect the action of the hand, would produce on carbonic gas.

Experiment E. Thermometer about 50°.

Into a glass jar filled with, and inverted in quicksilver, I introduced six ounces of carbonic gas, and exposed my hand to it, for the space of nine hours, in the manner, and with the precautions, before related. In that time, the air was reduced in quantity, to less than three ounces. A portion of the carbonic gas was examined, by the addition of lime water, before the experiment, when it was almost wholly absorbed; an unexaminable bubble only remained. When the remaining gas was examined by lime water, after the experiment, a considerable quantity of nitrogene,
L which

which doubtless exhaled from the hand, was found mixed with it.

I twice repeated this experiment, with similar events, though with rather less diminution, in the quantity of carbonic gas: it was however sufficiently evident, that the absorption of this gas, by the skin, was very copious and rapid.

The absorption of carbonic gas, makes it difficult to ascertain precisely, the quantity perspired, since that gas which is thrown out from the body by secretion, will probably be readmitted by absorption. I therefore wished to discover the quantity of carbonic gas, perspired in one hour.

Experiments F. Thermometer 30°.

The hand being retained one hour in five ounces of nitrous gas, no ascent nor depression of the quicksilver was remarked. On the introduction of Lime water into the glass, six drams of carbonic gas were absorbed.

In a similar experiment with atmospheric air, after the expiration of an hour, the quick-silver had rather risen, and three drams of carbonic gas, were discovered by lime water. In another experiment, in which hydrogenous gas was employed, four drams of carbonic gas, were found at the termination of an hour.

All the last related experiments, were performed in very hot weather. If two drams of carbonic gas were emitted in an hour, as the quantity usually obtained in five hours, was but one ounce, it would be a sufficient demonstration, of the absorption of a part of the air perspired. Neither are these experiments conclusive, as to the precise quantity of air emitted, for even in an hour, part of that which is exhaled, will be again imbibed. When I first attempted the experiments with carbonic gas, I supposed that the absorbents would receive it reluctantly, for I thought that matter, which was thrown out from the skin in such quantities, could neither be requisite, nor salutary to the body. The experi-

riment proved that I was mistaken, and there are reasons, which tend to shew the salubrity of this gas. When it is admitted into the stomach, it is generally found beneficial. When employed as a local application, its stimulus is useful, and when in combination with the blood, it probably produces, equally serviceable effects. The large quantity, in which it is generated, is in my opinion, an argument in proof of its utility. Its production, at first sight, may appear an act of necessity. In the decomposition of oxygenous gas, the carbonic, it might be supposed, must be generated, yet with such wisdom is the body constructed, that the benefit of any operation therein performed, seldom terminates in the production, of a single effect; but this result of a prior cause, becomes itself an agent, in the production of other useful consequences. Thus the decomposition of oxygenous gas, may be necessary, but the production of abundance of carbonic gas, is probably very salutary.

The experiments that have been related,
indistinctly

indistinctly shew, that a small quantity of one kind of air, when mixed with a larger proportion of another, can be abstracted from it, by the action of the animal body. This circumstance will be hereafter fully proved. I will now relate an experiment, that was made in support of this opinion, as it was performed beneath quicksilver, and in the same manner with those, which immediately precede it.

Experiment G. Thermometer between 60° and 70°.

Into a jar filled with, and inverted in quicksilver, three measures of nitrogenous gas, and three of carbonic were introduced; the two airs depressed the quicksilver, two inches and a half, and occupied the space of seven ounces of water. After five hours exposure of the hand, the air contained in the jar, filled the space of only five ounces and a half of water; on putting up lime water to this air, it was diminished to three ounces. In this experiment, one ounce and a half of carbonic gas appears to be removed, and half an ounce of nitrogenous; but if you admit that one ounce

of carbonic gas, was perspired during this experiment, and one-third of an ounce of nitrogenous, the quantity of air estimated to be absorbed, is increased, but the proportions, remain unaltered.

In the experiments with common air, I have mentioned, that it contained less oxygene after it had undergone the operation of the hand, than before it became the subject of experiment. A question here occurs; does this variation in proportion, arise from the addition of the one gas, or the removal of the other? That it is owing to absorption, will, I believe, be evident, from the following experiments. Although the addition made to any kind of air, cannot be accurately ascertained, when water is employed, yet if the hand removes any portion of air, that removal, will be ascertained by examination, neither does the experiment appear liable to deception. In the experiments next related, the air was confined by water; this gave me an opportunity of using larger vessels, and exposing a greater extent

extent of surface of the skin to the contact of the air. I forbore particularly to remark the quantity of air absorbed in the foregoing experiments, for though it corresponded to those, which I shall next relate, yet the correspondence was not uniform, and the degree of absorption was less evident.

Experiment H. Thermometer 60°.

I filled and inverted a jar in water, and put up into it twenty-four ounces by measure, of atmospheric air, to this the hand was exposed for twelve hours; the same precautions were used to avoid adding to, or taking from the air contained in the jar. The water had risen in the vessel, and about two ounces and a half of the air, were removed; that which remained was examined in the eudiometer, when two measures of it, and one of nitrous gas, filled the space of nearly two measures, and one-third of another; it therefore follows, that about one-half of the usual quantity of oxygenous gas, was removed from the other part of the atmosphere.

That there could be no addition of nitrogeneous gas capable of so greatly altering the proportions of these gases must, I think, be too evident, to need argument for its proof. Similar experiments, were afterwards made with correspondent events. In the experiments made under quicksilver, the abstraction of oxygen was equally evident, and considerable; it therefore appears, that the animal body is capable of taking away the oxygen, when in intimate mixture, with a much greater quantity of nitrogene. The avidity with which oxygen is absorbed, will be made still more conspicuously evident, by the following comparative experiment.

Experiment.

I filled and inverted two jars in water, into one I put twenty-four ounces by measure of nitrogenous gas, into the other, the like quantity of oxygenous. The hand was put into these airs alternately, and retained there for an hour each time: after it had been exposed to each, for eight hours, the water rose one-
eighth

eighth of an inch in the bottle, containing the nitrogenous gas, and nearly a whole inch in that containing the oxygene. On estimating the quantity removed, by weighing the water which filled the bottles to the different marks, it appeared that one-twentieth part only of the nitrogenous gas was removed, but one-third of the oxygenous gas, was gone. The remaining oxygenous gas was found to contain one-eighth more of nitrogenous gas, than before the experiment. I next examined the degree of celerity with which other gases would be imbibed.

Experiment.

Having filled and inverted a jar in water, and put into it thirteen ounces of nitrous gas, I retained my hand in this air, at different times, five hours, in which time three ounces were absorbed. My hand being retained, for as many hours in a like quantity of hydrogenous gas, not more than one ounce and a half was removed.

As

The removal of a quantity of oxygenous gas, from common air, is surely a curious circumstance; if this be the effect of an action, in the absorbing vessels, it must much exalt our ideas of their subtilty, and their aptitude, or disposition, to admit one species of matter, and to reject another. That the abstraction of one air, in preference to another, depends upon this cause, I believe will not, on reflection be doubted; it might indeed be suspected, that oxygenous gas was separated from the atmosphere, by the skin, as it is in the lungs, by chemical attraction: but it has been proved that carbonic gas is removed with equal celerity: and experiments on animal substances, shew in them a disposition rather to part with than to imbibe carbonic gas. The removal of this air, is therefore not likely to be the effect of chemical affinity. The different degrees of celerity with which other gases are admitted, seem to establish the opinion, that the removal of one kind of air in preference to another, is the effect of an active power, in the absorbing vessels.

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The experiments which have been related, satisfactorily prove, the quality of the aeriform perspiration ; perhaps the proportions may occasionally vary, but as nearly as I can determine, it consists of rather more than two parts of carbonic, with the remainder of nitrogenous gas. The quantity of the matter perspired is less certainly ascertained ; in one hour I obtained four drams of carbonic gas : but it should be remembered, that these experiments were made in very hot weather ; and it also deserves notice that the quantity of the cutaneous perspiration is subject to great variety. In every experiment, absorption was found to be equal to perspiration, in many it was much more copious ; especially, when the air to which the skin was exposed, was salutary to the constitution. The oxygenous and carbonic gases are very readily imbibed ; whilst the nitrous, hydrogenous, and nitrogenous gases, tardily gain admittance into the absorbing vessels. In the experiment marked G. from about half of the surface of the hand, two ounces and a half of carbonic gas were absorbed in five hours : in those experiments marked H, from the
hand

hand and wrift, there was imbibed,

In eight hours	8 ounces of oxygenous gas.
In five hours	3 do. - - nitrous gas.
In five hours	1½ do. - - hydrogenous gas.
In eight hours	1 do. - - nitrogenous gas.

I next endeavoured to ascertain the quantity, and quality, of the aqueous perspiration.

Experiment I Thermometer 65°.

I introduced my hand, and forearm, into a glass jar, covered with bladder; an aperture was left in the bladder, to admit my arm, round which the bladder was tied; so that the ascent of any vapour was prevented. In six hours, I procured nearly three drams of limpid tasteless water. The quantity collected, corresponds with the result of Mr. Cruikshank's experiments, who obtained the water of perspiration in the same manner. Half of this liquid was evaporated by a gentle heat, there remained a small residue on the glass, which had a very slight taste of salt. The other half was suffered to stand many days, in which time, no change

change appeared: it did not then alter the colour of the vegetable blue. Into one portion of this watry liquor, marine acid was drop'd, which caused no coagulation, or precipitation, of animal matter: into the other, some caustic alkali was poured, which produced no visible effect. I therefore conclude that the water of perspiration, in a state of health contains little of any thing, except a very small portion of salt. The matter of perspiration has sometimes a sour odour; whether it really contains an acid, I have had no opportunity of determining. It is a common observation, that the skin frequently has a salt taste; doubtless if saline matter abounds in the fluids, it will be secreted in greater quantity.

Perspiration is generally said to be sensible, or insensible, perhaps it may be better distinguished as aeriform, or watery. It may be expected, that a general estimate of the quantity of this secretion, should be attempted; but the difficulties which oppose any accuracy of statement are considerable. In these experiments

periments, the process was not continued under its usual circumstances; the arm was surrounded by water, or quicksilver; and when in the latter fluid, the circulation was in some degree interrupted by its ascension, and pressure, against the edge of the jar.—For the uncertainty, which these circumstances occasion, allowance can be made, but before an estimate of the quantity of perspiration be attempted, the extent of the surface of the body, should be known. Mr. Cruikshank supposes the extent of the hand, to be to that of the body, as one to sixty: it is much more, according to my computation.

After ineffectually endeavouring in different ways to measure the surface of the body; I concluded that I should approach nearest to its true extent, by measuring the circumference of the trunk, and limbs, at different parts, and having thus obtained the mean circumference; I could then calculate the extent of their surface, as if they were cylinders; the dimensions of which, were ascertained. The surface of the head, hand, and foot, I computed

puted, by applying paper cut as the occasion required, over these parts: afterwards placing the separate pieces of paper, so as to form an extended plain, I measured its extent. I shall mention these measurements, that the reader may correct them, if he thinks them erroneous. If a man be five feet six inches high, I will suppose the mean circumference of the trunk of his body, to be thirty-three inches, and its length from the top of the sternum, to the os pubis, twenty-two inches.

	Square Inches.
The extent of surface of the trunk will therefore be	726
The circumference of the neck 13 inches, its length from the sternum to the chin 3 inches - - -	39
The surface of the head, and back of the neck -	286
The mean circumference of the arm 10 inches, its length 12. Surface of both arms - - -	240
The mean circumference of the forearm 8 inches, its length 10. Surface of both forearms - -	160
The surface of the hands and wrists measuring to the extremities of the bones of the forearm -	140
The mean circumference of the thigh 17 inches, its length 16. Surface of both thighs - - -	544
The mean circumference of the leg 11 inches, its length 14. Surface of both legs - - -	308
Surface of both feet - - -	182
Allow for folds of the skin, inequalities of surface, &c.	175
The extent of the surface of the body will be -	2700
The	

The superficial extent of the hand, and wrist, according to this calculation, is to that of the body, as one to about thirty-eight and a half.

In three experiments, marked F, the least quantity of carbonic gas emitted from the hand, in one hour, was three drams by measure; it may be supposed that the heat of the weather increased the secretion from the skin, let us therefore consider two drams as the ordinary quantity. If then the perspiration of all parts were equal, seventy-seven dram measures of carbonic gas and one third of that quantity of nitrogenous gas, would be emitted from the body, in the space of one hour. If we also suppose perspiration to be at all times equal, nearly three gallons of air, would be thrown out from the body in the course of one day.—Altho' the quantity of air perspired is so large, yet the weight of the body will not be much altered by its loss, it is the aqueous perspiration, by which this will be principally diminished. When the thermometer was between 60° and 70° , I obtained about thirty grains of the fluid from my hand and

part

part of the forearm in an hour; the surface from which, this secretion was made, I compute to be one twenty fifth part of the extent of the body. The suppositions being allowed, that perspiration is at all times, and in every part, equal: about two pounds and a half is, the loss of water which the body would in one hot day sustain. In most of the experiments which I have made, the absorption of air, was equal to the perspiration; in many, it was much greater, especially if the air was salubrious, to which, the skin was exposed. The experiments marked H, make it appear probable, that if the naked body was exposed to fresh currents of the atmosphere, that only the oxygenous part would be absorbed; the decomposition of which, in the body, would produce an increase of animal heat; which might in some, degree counterballance the loss sustained by the exposure. Our cloathing probably prevents, in some degree, this effect, and perhaps makes it less necessary. If the perspired carbonic gas be confined by our garments, it seems likely, that it will be taken up again by the absorbents. Whether the body

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does

does usually imbibe water from the atmosphere, adequate to the loss sustained by aqueous perspiration, is uncertain. I am inclined to suppose, that the absorption of air from the skin, is greater than the secretion. The great quantity of water, which the skin can occasionally imbibe from the atmosphere, is evinced by people in a dropsy, and has been of late well shewn, by Mr. Ford, in the second volume of the medical communications.

I next examined briefly, and in a comparative manner, the effects produced by the process of respiration; because I perceived in writers, some disagreement of opinion on this subject. I first collected the water exhaled from the lungs, by breathing into a deep bottle covered with bladder, in the middle of which, was inserted a tube, through which I breathed. From the bottom of the bottle another tube proceeded, which ascended by its side and terminated on a level with its top. The air which I impelled into the bottle, found an exit by this tube, but the water being condensed, became too heavy for ascension, and remained
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in the bottle ; thus the uneasiness which Mr. Cruikshank experienced, in performing a similar experiment, was avoided. In an hour, I collected nearly three drams of insipid, but not perfectly limpid water ; half of this liquor was evaporated : a slight crust remained on the glass, which was not salt, but had a burnt smell. I suffered the remainder to stand many days, but could perceive little odour from putrefaction, it changed however the vegetable blue to a faint green. I added to one portion of this liquor some caustic alkali, which had no visible effect ; I drop'd marine acid into the other, it became cloudy, and soon a filamentary deposit was made, which resembled mucus when precipitated : this precipitate dissolved with difficulty in caustic alkali. In the water exhaled from the lungs, I discovered no salt, but it seemed mixed with a mucous like matter, which gave it some viscosity.

I proceeded to enquire into the changes, which respiration produces in the air, that is breathed. Doctor Goodwyn in his experimental

enquiry, has stated, that the quantity of nitrogenous gas, is neither augmented, nor diminished, by respiration. That nitrogenous gas is emitted from the lungs, I believe is the general opinion, which I think altho' not exactly ascertained, is yet rendered probable by the following experiments.

Experiments.

Whilst respiration was performed, as nearly as possible, in the ordinary manner; I drew in twelve cubic inches of atmospheric air, and expelled the same quantity into a glass vessel, filled and inverted in quicksilver. One eighth part of this air was absorbed by lime water; the remainder was examined in the eudiometer, and it was found that only one twelfth part of the oxygenous gas, was taken away.

Having filled and inverted a jar in quicksilver, into which, was introduced a bent tube also filled with quicksilver; I inspired a moderate quantity of air, and retained it untill more than a slight uneasiness was felt. I then expelled the air from the lungs into the jar. Of this air, one eighth part was absorbed by lime water; the remainder being examined
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in the eudiometer, was found to be deprived of less than one-sixth part of its oxygenous gas.

I have related these two experiments out of many which were made, as they seem to shew the greatest, and least degree, in which the atmosphere is deprived of oxygene by the lungs.

The change produced in the air inspired, must doubtless vary according to its quantity, and the time of its continuance in the lungs: I purposely retained the air in the last experiment, longer than usual, that the deprivation of oxygene might be remarkable. I think the quantity abstracted, varies between one-twelfth and one sixth part. Doctor Goodwyn makes the diminution of oxygene, much more considerable, and has estimated the rate of its abstraction, by successive respirations. In the first respirations four parts out of eighteen, were removed: after the fifth, eleven parts of the oxygene had been imbibed from the lungs. In every experiment, which I made, the removal of oxygene, by the first respiration was less, and by the fifth frequently more, than Doctor Goodwyn has related.

Many eminent experimentalists have asserted, that the bulk of the air inspired, is diminished in the lungs; and that less air can be returned, than was admitted. To me the contrary appeared to be true in experiment, and I believe reflection will convince any one, that it is so in reality. For, if between twelve and thirteen parts of carbonic gas, be given out from the lungs, and if only between two and three parts of oxygenous gas, be taken in, whilst the air remains in those bodies; there must be, either an addition made to the bulk of the air; or a considerable quantity of nitrogenous gas, must be dissipated in an inexplicable manner. To set this in a clear point of view, I shall state these circumstances after the manner of Doctor Goodwyn. I shall take that experiment, in which one-sixth part of the oxygene, was abstracted.

If the air inspired contains of

nitrogenous gas.	Parts	80
oxygenous do.	do.	18
carbonic do.	do.	2
		100

And

And the same quantity of air be expired,

it contains, of nitrogenous gas. Parts 72½

oxygenous do. do. 15

carbonic do. do. 12½

100

But the expulsion of the same quantity of air, is the act of the person who makes the experiment, and not the natural effect of the process of respiration. Were this statement, a correct representation of the change produced; it would excite surprize to find so large a quantity of nitrogene removed. It could not like the oxygene be imbibed by the blood: and the continuance of the air in the lungs, is too short to admit of its removal by the absorbing vessels. The absorbents of the skin, reluctantly admit nitrogenous gas; and analogy induces us to suppose, that those vessels in the lungs, would reject it equally. The removal of nitrogenous gas by the lungs, in any evident quantity, is so contrary to analogy and reason, that the truth of any experiment indicating such abstraction, ought to be suspected.

That the carbonic gas, discoverable in the air expelled from the lungs, is exhaled from the pulmonary vessels, and increases the bulk of the expired air, appears to me sufficiently evident.

At one time, physiologists believed that the inspired oxygenous gas, contributed to the production of the carbonic gas, found in the air expired. This opinion perhaps still prevails in the minds of some people. The quantity of oxygene imbibed by the lungs is however too small, to contribute to the formation of so much carbonic gas, as we find given out from those bodies. The experiments marked A, clearly prove, that the exhaling vessels of the skin, emit carbonic gas, in a state of complete formation. And doubtless, those of the lungs perform a similar office.

If then but little nitrogenous gas, be removed from the air inspired, and if the carbonic gas be emitted from the pulmonary vessels, and augments the bulk of the air expired;

expired; the change produced by respiration, might be thus stated,

The air inspired contains of

nitrogenous gas.	Parts	80
oxygenous do.	do.	18
carbonic do.	do.	2
		100

The air expired, contains of

nitrogenous gas.	Parts.	80
oxygenous do.	do.	15
carbonic do.	do.	12½
		107½

To me however it appeared, that the quantity of air expired, was nearly equal to that inspired, when the experiment was performed beneath water, and when, the carbonic gas could not increase the quantity of emitted air. Of the truth, or falsity, of such an experiment, any one may readily form his own opinion; but if it be admitted, that about two parts in a hundred disappear; then, the effects of respiration, might be thus stated:

The

The air inspired contains of			
nitrogenous gas.	Parts.	80	
oxygenous do.	do.	18	
		98	

The air expired contains of			
nitrogenous gas.	Parts	81	
oxygenous do.	do.	15	
		96	

Add also carbonic gas		
absorbed by the water		12½
		108½

And you will thus obtain an account, which in my opinion, is the nearest representation of the change that respiration produces. As a large quantity of air, is always retained in the lungs, it is probable that the absorbing vessels, do like those of the skin, constantly imbibe it: and this perhaps, is the cause of the diminution of the quantity of air, in which, an animal has been suffered to perish. It seems also probable, that the emission of air, from the bodies of dying animals will be much diminished; but the function of absorption

forption will be less impeded, by the declining powers of the sanguiferous system. I think it unnecessary, to attempt to calculate the loss or acquisition of air, and water, to which the body is subjected, by the process of respiration: it appears sufficient, to remark, that the air and water, which enter into the composition of the animal body, undergo from the actions of the skin and lungs, constant and rapid changes; whilst the other more essentially constituent parts, of the animal, are the subjects of a much more gradual alteration.

The similarity of the office, performed by the skin, and the lungs, explains, in my opinion, many circumstances, observable in the causes, and cure of pulmonary consumption. The people most liable to such complaints, are those, whose constitutions are feeble, and whose thorax is of scanty dimensions. If the cutaneous perspiration of weakly people, be checked by exposure to cold, it is tardily restored to its former state; the constriction which exists on the surface of the body, is with difficulty overcome, owing to

the deficiency of power, in the heart, and arteries. The consequence will be, a plethoric state of the vessels; from a retention of that great quantity of matter, which ought to be emitted from the surface. For the relief of this plenitude, other discharges will be substituted: common experience informs us of this fact, that weakly people readily (as it is expressed) take cold, the consequence of which is, discharges from the nostrils, the bowels, or the lungs. But if people of vigorous health, accidentally suffer any constriction of the cutaneous vessels, the internal powers possess strength to remove it, and to propel again the fluids through the exhalants of the surface.

When perspiration is diminished, the determination of fluids to the lungs, is particularly to be expected; because the copious secretion which is made from those parts, will relieve plethora; and because, that secretion is similar to the one which has been suppressed. The blood will become surcharged with air, to which the lungs only
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can afford an outlet. It is as much to be expected, that when perspiration is suppressed, the office of the lungs will be increased, as that, performed by the kidneys: by means of the latter, the blood is freed from the superabundant water; by the former, from the retained air.

Thus an accumulation of fluids in the pulmonary vessels, will ensue, which will be more considerable, and more likely to produce disease, when the thorax is of scanty dimensions; and the transmission of blood through the lungs, in consequence, difficult. This plenitude of the arterial system, in the lungs, probably will produce inflammation; the degree of which will vary, as the exciting cause, and the constitution of the patient determine. I am inclined on reflection to believe, that a deficient performance of the functions of the skin, is the principal cause of pulmonary consumption.

This supposition explains, why the inhabitants of this variable climate, especially those

those of weakly constitutions, and malformed chests, are so peculiarly obnoxious to such complaints. This supposition also shews, in what manner, preventing the effects of accidental cold, by flannel garments, or by removal to a warmer climate, is so eminently beneficial. The fluids are invited by warmth to the surface, and the functions of the skin are encouraged. The lungs are relieved from oppression, and left free to the exertion of the restorative powers of the constitution.

That scrofula may be regarded as a cause of pulmonary pthisis, I will readily admit; if it produces disease of the bronchial glands, the consequent obstruction to absorption from the lungs, will afford a kind of stimulus, likely to induce that indolent inflammation, in which, the disease generally consists. The opinion that scrofula is a cause of pthisis is confirmed by the appearance of some tubercles, which contain a matter resembling chalk and water, and which is often found in other cases, to be the product of scrofulous inflammation. If the disease of the lungs is
admitted

admitted to be, in many instances, of a peculiar nature, it is still probably occasioned by the same exciting cause, a diminution of the functions of the skin, and a consequent determination of fluids to the lungs. The debility of the sanguiferous system of vessels, which is observable in scrofulous patients, may also, for the reasons before stated, render them more liable to this disorder. But from observing the number of patients who die of pthisis, who have no appearance of struma, I am induced to believe, that this cause is less frequent than is generally supposed. At one time I examined the bodies of many people, who died consumptive: in a small number I found the lungs still capable of admitting a moderate quantity of air; in such cases, either the cells were larger, appearing as if many cells were laid into one by ulceration, or the air had entered into the cavities of vomicae, or abscesses; but in the greater number of cases, the lungs admitted but of slight inflation, instead of being of a light and spongy texture, and permeable by air, they were made solid, being bestudded by large or small tubercles,

or

or they were to a considerable extent thickened, and rendered uniformly dense. In those cases, where the lungs remained irregularly hollow, the disease appears to be the effect of a more active inflammation; ulceration has taken place, abscesses have formed, and the patient has suffered considerably from pain, irritation, and discharge.—But the process by which the lungs are made solid, seems to be a languid, indolent inflammation, unaccompanied by a degree of pain, that demands much attention. It is of the same nature with that inflammation, which produces farcomatous growths of glandular parts, and induration of the cellular substance. This indolent inflammation is frequently regarded as proceeding from scrophula; that it often exists distinctly, I am convinced. A scrophulous constitution may indeed be more obnoxious to languid inflammation; to this, however, it ought not be attributed as a cause; since its separate existence, proves its independence.

Where the lungs are still capable of admitting

ting air, could the irritation and discharge be lessened, or removed; it is probable, that life might still be supported: but if the lungs, by disease, be made nearly solid; their office can be no longer performed, and death must be inevitable.

This disease, which is the effect of chronic inflammation, steals insidiously upon the patient, and is often established, beyond the possibility of removal, before its attack is even suspected. Yet I think, by physicians, its presence may be early known, since its effects are, to make the lungs less capacious. The capacity of the lungs, admits of mensuration: let a person inspire as largely as he is able; let him then make a forcible and complete expiration; and expel the air into a receiving vessel: thus, the additional quantity of air, that the lungs can receive by the most ample inspiration, is made visible. A glass bottle may be filled and inverted in water, and the patient may expire through a bent tube. Muscular debility, or spasm, may occasionally make the result of this experiment doubtful,

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yet

yet in general, I believe it will afford useful information.

In the cure of almost every disease, the removal of the producing cause, seems to be the primary object. If, as I believe, a deficient performance of the functions of the skin, be the principal cause of pulmonary consumption; the excitement of cutaneous perspiration, requires particular attention. Maintaining an equable warmth, either from climate, or from cloathing, is one way of effecting this purpose: giving strength to the vascular system, is another; and which, in my opinion, has not met with sufficient attention; at least the principle, on which its utility depends, has not been sufficiently explained. All parts of the skin may be regarded, as the extremities of the body: if the heart and arteries be wanting in power, the circulation, and secretion of this remote part, will be languid, and deficient at all times; and will be liable to suspension, from trivial causes. The maintenance of the strength of the vital powers, appears to me, essential to the cure of consumption.

sumption. The skin cannot perform its office unless these powers be supported; and if this be deficient, pulmonary plethora will be inevitable. The relief which emetics afford in pulmonary complaints, appears to arise from the production of cutaneous exhalation, which they occasion.

But if by promoting cutaneous secretion the cause of the disease is removed, still more remains to be effected: the lungs perhaps have already been thickened, or beset by tubercles; how then, is the diminution of this disease to attempted. In other parts, we frequently endeavour to accomplish a similar intention, by means reductive of the patient's strength: by purges, by mercurials, and other medicines: but all means, which debilitate, should be cautiously employed. It is however the nature of the disease, that I have endeavoured to examine; in the practice, I will not presume to recommend.

I shall here present the reader with an account of an uncommonly constructed heart:

in consequence of which peculiarity, the circulation of the blood was often irregularly performed; the effects of which, I think tend to illustrate the functions of the lungs. As the body is formed with perfect wisdom, any unusual construction of a part, is likely to be less adapted to the performance of its peculiar functions, and less concordant to the actions, and design of the whole. Such deviations from usual structure, do occur in unimportant parts, without essential injury; and are frequently observed: but in an organ so eminently important as the heart, any malformation can hardly be supposed to exist, and life to continue: nor do I know that any case similar to that, which I shall now relate, is to be found in the records of anatomy. I have annexed to the account, an engraving of the heart, the better to illustrate its peculiarities.

Mr. Adams requested me to assist him, in opening the body of a child who died at a little more than two years of age, and of the state of whose health, whilst he lived, Mr. Adams has given me the following account.

“ The first paroxysms of irregular respiration, were observed about a fortnight after birth. They were however neither violent in degree, nor long in duration. For some time after, they were periodically every third day, and for the last twelve months of the child’s life usually once a day. The following were the symptoms: the skin which from his earliest infancy had been remarked to have a darker tinge than common, became unusually blue. A coldness which was so habitual, as to induce his parents always to clothe him in flannel, was much increased. The child seemed to suffer much uneasiness, about his chest, and shewed a wish to be laid with his face downward; if this were not immediately complied with, he contrived to turn himself to that posture. He then gradually, and with apparent pain and exertion, expelled air from his chest; remaining without inspiration for a much longer period, than an adult could, without suffocation. After this, suddenly inspiring, he immediately expelled the air as at first; this exertion was attended with a

“ kind of scream. In this manner he conti-
“ nued respiring with immoderately long in-
“ tervals, for near a quarter of an hour : after
“ which he breathed naturally, but always
“ quickly ; and thus usually the paroxysm
“ subsided. But sometimes the symptoms
“ have been renewed, after a few minutes of
“ ordinary respiration, and a succession of par-
“ oxysms, has continued through the day.

“ Though the child was inattentive to sur-
“ rounding objects, from the moment he was
“ seized, till his recovery, yet this seemed
“ rather the effect of his sufferings, than a
“ privation of intellect. When long without
“ a fit, he was observed not to be so well ei-
“ ther in health, or spirits. If the paroxysm
“ was very severe, the colour of his skin was
“ proportionably livid, and after it was over,
“ mended in proportion : his lips which at
“ all other times were blue, acquiring a reddish
“ tint which lasted for an hour or two. His
“ pulse was always regular, but extremely feeble
“ and

“ and quick, was sometimes so obscure, as to be
“ felt with difficulty.

“ For the three days preceding the Child’s
“ death he had no fit, but on the fourth
“ morning his respiration was again irregular,
“ though not exactly in the usual manner.
“ Respiration was performed at shorter inter-
“ vals, and with less exertion than was com-
“ mon; but the skin became pale, and the
“ powers by which the circulation of the
“ blood is performed, gradually ceased to act.”

The length of the body, was rather less than that of a healthy child of the same age; it was slender, but very well formed: the muscles were moderately large and firm: there was no deficiency of fat: neither disease, nor peculiarity of formation, were observed in any other part of the body, except the thorax. The lungs were healthy, but the structure of the heart was uncommon. The right auricle of the heart, being first laid open, was found to be much larger than usual: and the foramen ovale was perfectly open. The cavity
of

of the right ventricle, was next exposed; it was of uncommon size, and its sides resembled those of the left ventricle, in bulk, and apparent strength. From this ventricle, a large vessel proceeded, in the usual course of the pulmonary artery: I laid it open to some extent, but it proved to be the aorta. From its origin in the ventricle, it extended in an arched form, towards the left side; then passed behind the lungs, and pursued its usual course. The aorta was more capacious than is common; it gave off the coronary, carotid and subclavian arteries in the usual manner. The communication between the ventricle and this artery, was large and direct, and it appeared, that it would allow an easy passage to the blood, on the contraction of the ventricle. The pulmonary artery also arose from the right ventricle, but the communicating orifice was small, and the artery was one-third less than its usual size: its sides were uncommonly thin, resembling those of a vein. In the engraving, the aorta is expanded, and inclined to the right side: but before its displacement, it was turned towards the left, going before the pulmonary artery, and concealing that vessel.

The

The blood was returned from the lungs, in the usual manner, by four pulmonary veins, to the left auricle. The dimensions of the left auricle and ventricle, were smaller than common: they were certainly one-third less than the corresponding cavities of the right side of the heart. No artery proceeded from the left ventricle, but there was an opening in the upper part of the septum ventriculorum, by which the blood could be projected into the aorta. In the engraving, as the aorta is displaced, being expanded, and inclined to the right-side, this communication appears more direct than it was in reality.

A heart thus constructed, was well calculated for carrying on the foetal circulation. Since both ventricles could project their blood into the aorta. The pulmonary artery would of course receive no more blood, than what in the uninflated state of the lungs, it was capable of transmitting. When respiration took place, a quantity of blood adequate to its size, would be distributed by the pulmonary artery.

The

The dimensions of that vessel, as well as those, of the left side of the heart, shew that the pulmonary circulation was at all times, in some degree deficient. When the right ventricle contracted, a considerable quantity of venal blood must have been projected into the aorta; for the passage into that vessel, appeared more patent and direct, than that, which led to the pulmonary artery. From attentive examination of this heart, I am induced to believe, that at each contraction of the ventricles, nearly an equal quantity of venal and arterial blood, was impelled into the aorta, for the supply of the body. This occasioned the body to be, at all times, more cold and livid than usual, since the blood was not sufficiently oxygenated, to produce the usual redness and heat,

That the structure of the heart, would admit of the circulation being performed, without the continuance of respiration, is sufficiently evident: that during the paroxysms of irregular respiration, the functions of the lungs were much interrupted, the lividness and coldness

ness of the body shew. The pulmonary circulation, though much diminished, was still probably in no inconsiderable degree continued; so that we cannot pretend, from this case, to determine, what would be the effect of the distribution of venal blood throughout the body. It is, however, apparent, that the deficiency of oxygenation of the blood, did cause great lividity and coldness of the body; and was attended with a considerable diminution of the strength of the vital powers: this was indicated by the debility of the pulse, and by the degree of languor, which the child always experienced, on its recovery from this peculiar state of impeded respiration,

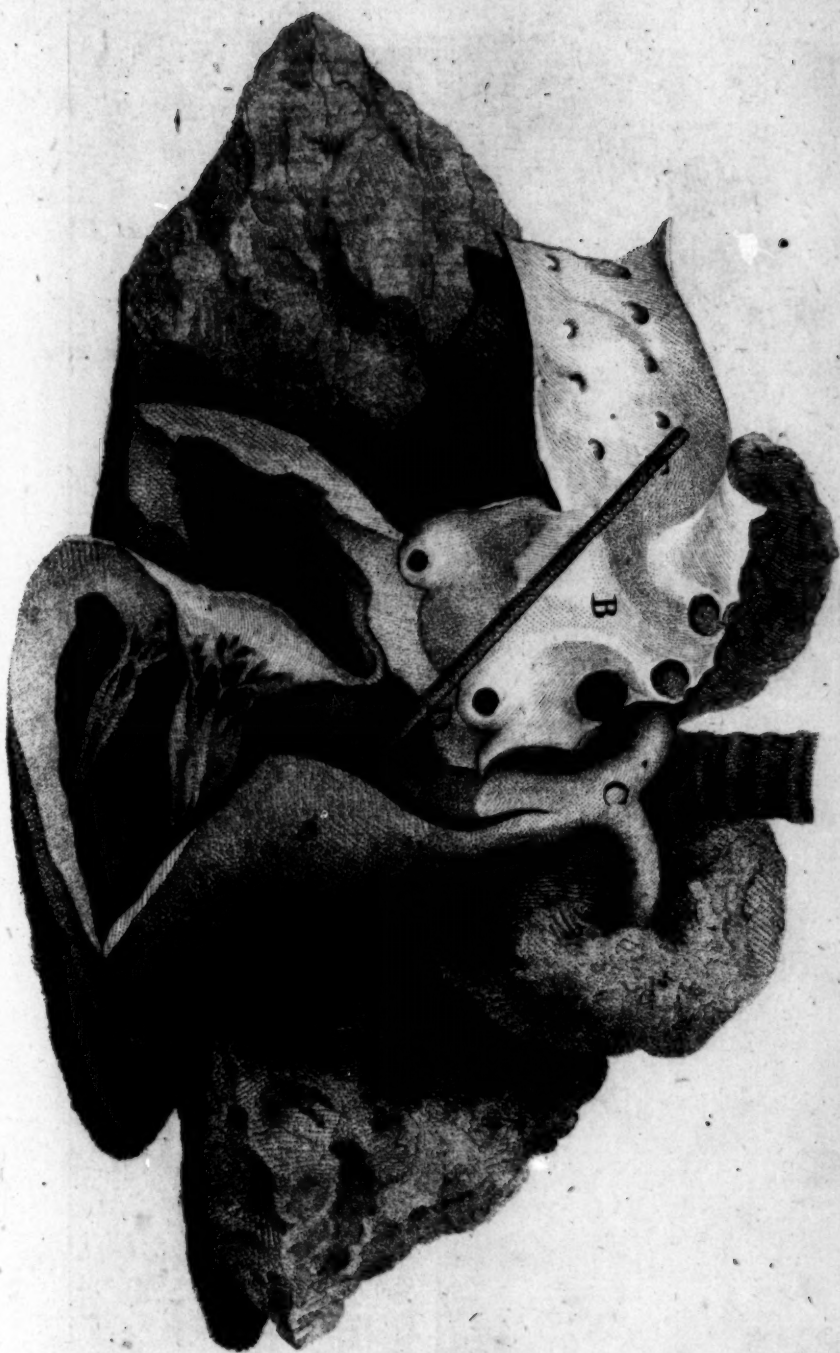
Cases of a familiar deficiency of pulmonary circulation, from a different cause, are related by Doctor Hunter, in the sixth volume of the Medical Observations and Inquiries.

EXPLANATION OF THE FIRST PLATE.

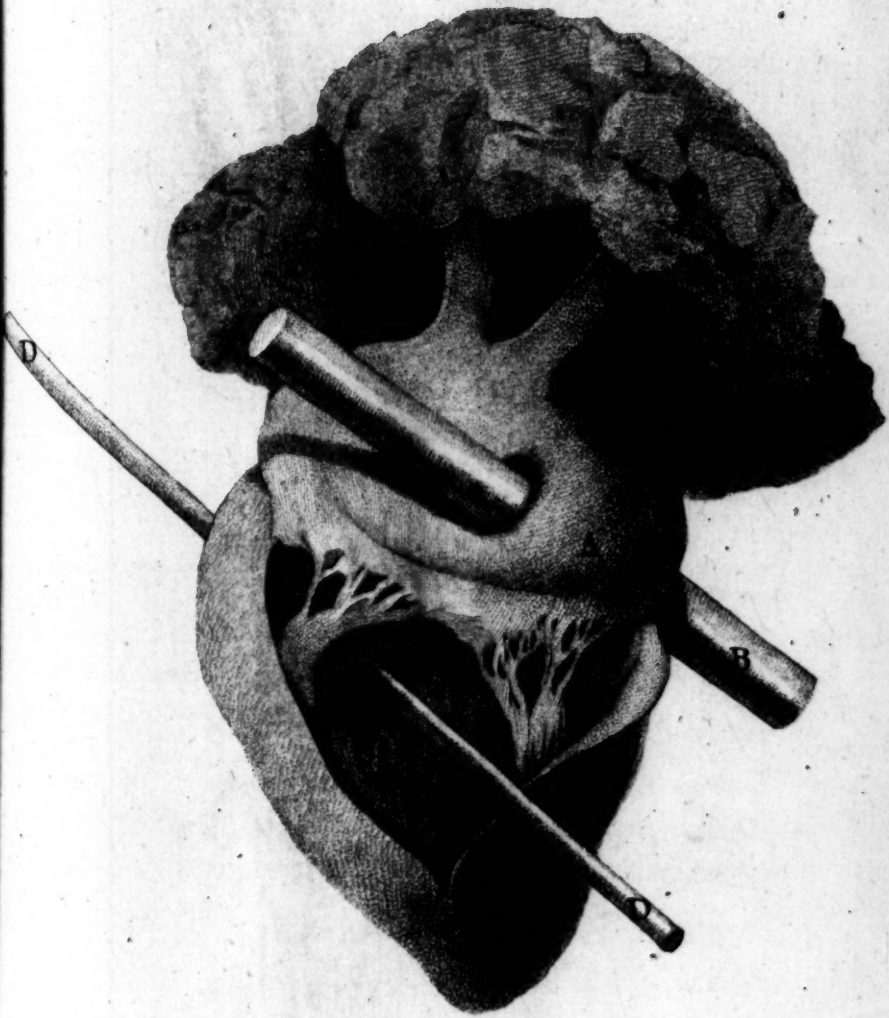
- AA** The right ventricle laid open.
- B** The aorta expanded, and inclined to the right side : before its displacement, it went before the pulmonary artery, and concealed that vessel.
- C** The pulmonary artery, which appeared one-third less than usual.
- D** The opening in the septum-ventriculorum.
- E** A probe passed through that opening, into the left ventricle.

EXPLANATION OF THE SECOND PLATE,

- A** The left auricle laid open.
- B** A piece of wood put through the foramen ovale.
- C** The left ventricle. Both the cavities of the left side of the heart, were one-third less than the corresponding cavities, of the right side.









AN

ESSAY

ON THE

ILL CONSEQUENCES SOMETIMES SUC-
CEEDING TO VENÆSECTION.

THE public are much indebted to Mr. Hunter, for a judicious account of the appearance, and effects of the inflammation of the vein, which sometimes succeeds to venæsection. The ill consequences which occasionally follow that operation, are numerous and dissimilar; and they have never I believe been clearly, and collectively stated, and explained. The cases recorded of such complaints, are dispersed in various periodical publications; and frequently, the nature of
the

the disease, appears to be not understood by the person, who relates its history. In proportion as I have seen, more varieties of these diseases, my own knowledge of them, has become more clear, and simple; and as I believe, I can communicate useful information, I have ventured to offer to the public, the following observations and opinions. I have been also incited to this task, because the account which Mr. Bell has given of these complaints, appears to me confused; and the practice recommended, improper. I am hurt to censure the works of any author, but this either must be done, or injurious error must remain uncontradicted.

When from want of attention, or from other causes, the wound inflicted in venæsection does not speedily unite; the motions of the arm, occasion attrition of its sides, against each other, and inflammation of the wounded, or contiguous parts, is likely to ensue. I shall give a brief account of these different complaints, in the order in which, I believe they most frequently happen.

Of

*Of inflammation of the integuments, and
subjacent cellular substances.*

The inflammation, and suppuration of the cellular substance in which the vein lies, is the most frequent occurrence. Of this every surgeon must have seen repeated instances, they may also have remarked, that on the subsidence of this inflammation, the tube of the vein is free from induration: neither does the state of any of the surrounding parts, indicate their previous participation in the disease. The nature of every excited inflammation, will vary as the cause which produced it, and the constitution of the patient determine, it will therefore be unnecessary, to particularly notice the varieties of its appearance. Sometimes the inflammation will be more indolent, and will produce a circumscribed, and slowly suppurating tumour. Sometimes it will be more diffused, partaking more of the nature of erysipelas: and sometimes its violence, and rapid termination, will evidently distinguish it to be a phlegmon.

If

If the lancet with which, the patient was bled, should have been bad, if it lacerated rather than cut the parts, through which it passed; if the constitution of the patient be irritable, and more particularly, if sufficient attention be not paid to procure the union of the divided parts, but the motion of the arm be allowed: the irritation, which the friction of the opposite edges of the wound must occasion; will most probably excite inflammation. The treatment proper to be pursued in this complaint, is manifest, and distinguished by no peculiarity; I shall therefore postpone what I have to say on that subject, until I have noticed the other varieties of these diseases.

Of inflammation of the absorbing vessels.

The next frequent complaint, which I have seen, is inflammation of the absorbents: it however sometimes accidentally happens, that one surgeon meets with many cases of a similar nature, so that were he to judge merely from his own observation; he might conclude,
that

that disease to be common; when the collected experience of others, would determine it to be a rare occurrence. I am inclined to suspect; that my observation has been thus partial; since Mr. Hunter has not publicly noticed this complaint. I think I cannot give a better history, of the commencement; appearances; and event of this disease; than by relating three cases; of the circumstances of which, I took an account. It is right however to mention, that I have seen two others, of which, I took no minutes; and which, I am unwilling to relate, only from recollection.

CASE:

A lady was bled in the vena mediana basilica; the wound did not heal, nor was sufficient attention paid to preserve the arm quiet. Eight days afterwards, I was consulted, in consequence of the patient being alarmed, by the appearance of two swellings; one was situated about the middle of the arm, over the large vessels, the other on the forearm, about the mid space between the elbow and wrist,

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in

in the integuments above the flexor muscles. The upper swelling measured rather more in circumference, than an egg, the other, was of smaller dimensions; they were not very painful, they were moderately firm in their texture, and so exactly resembled those tumours, which form round irritated lymphatics; that no doubt could be entertained of their nature. The orifice made by the lancet, was not healed, the integuments for about one-fourth of an inch surrounding it, were in a slight degree inflamed, and thickened. No induration of the venal tube could be distinguished, either at this time, or after the subsidence of inflammation.

The account, which I obtained from the patient, of the attack of this complaint; was, that the wound inflamed, became painful, and discharged matter; that the gentleman by whom, she was bled, had dressed it with salve, but did not restrain her from using her arm; that about five days after the operation, she had felt pains shooting from the orifice, in
lines,

lines, up and down her arm, and upon pressing in the course of this pain, its degree was increased. This account induced me to examine the arm attentively, and I could plainly feel two indurated absorbents, leading to the superior tumour, but could not perceive any, extending to the lower one. The wounded part, was dressed with mild salve; a bread and milk poultice was applied to both tumours, and the arm was supported by a sling, without motion, or exertion. The integuments surrounding the orifice, lost their disposition to inflame, and the wound gradually healed; during five days, the tumours underwent no evident alteration; the poultice was changed to one of bread, water, and acetum lythargiritum, cum tinctura opii, under which, they quickly diminished and dispersed.

CASE.

A man about 35 years of age, was admitted into St. Bartholomew's Hospital, under the care of Mr. Pott: he had been bled in the country, about a fortnight before his admission; since that time he had been extremely

ill, and was with difficulty conveyed to London. The state in which he was admitted, I shall describe: His whole arm was greatly swollen, the wound made by the lancet, was not united, the parts immediately surrounding it, did not seem to be affected, by distinct inflammation; but partook of the general tumefaction. Two large abscesses had formed, one, situated near the inner edge of the biceps muscle, about the middle of the arm; and the other, on the inside of the forearm. The patient told us that he had been bled, on account of a pain in his side; that the orifice, instead of healing had festered, that he had for a time, pursued his daily employment, notwithstanding the pain which he suffered; that this, however soon became too violent to be endured, the swelling, and pain extended towards the armpit, where the glands became enlarged. Inflammation next attacked the forearm, and after suffering extreme pain and fever; these abscesses had formed, and since that time, his illness and pain had in some degree abated. Mr. Pott opened both abscesses, and directed his

his whole arm to be covered with a poultice. The patient was kept in bed, and medicines likely to alleviate inflammation, were prescribed. In about four weeks, the arm was reduced nearly to its natural dimensions. The orifice, through which he was bled had united, and the wounds, by which the abscesses had been opened, were nearly healed. The parts surrounding them, however, still remained thickened, and also all the integuments on the inside of the arm. In these thickened integuments, three-chord-like substances, evidently absorbents, were to be distinguished; they extended from the punctured part, to the superior abscess, and again above this, two were continued even to the axilla. Two other indurated absorbents, also were extended from the punctured part, to the inferior abscess. The punctured vein being attentively examined, was found to be a little thickened, both above, and below the orifice; it had however, no connection with these chord-like substances, they were superficial, and their appearance, course, and every other circumstance, clearly shewed

them to be indurated absorbents. The hard-
 nels of these vessels, and of the integuments
 had much diminished, and the patient had re-
 gained the strength of his arm, before he was
 discharged from the Hospital.

CASE.

A poor man was bled, in one of the bleeding-
 shops of this city. His operator dipped some
 rag in the blood which he had taken, applied
 it to the orifice, and bound it on the arm, with
 a tape. The patient felt much pain in the
 wound, even from the time of the operation,
 and experienced much difficulty in moving his
 arm. As the rag stuck closely to the orifice,
 he was unwilling to remove it; however, on
 the third day, the violence of the pain, in-
 duced him to take it off: he then found the
 parts surrounding the puncture, inflamed and
 hardened. The patient had also suffered much
 pain, which extended towards the axilla, and
 one of the glands there, was swollen. He
 anointed the arm with some ointment, but the
 pain

pain so increased, that he could scarcely bear it to touch his side. The integuments about the middle of the arm, were elevated by a tumour, which was painful when pressed, the base of it was not circumscribed, but was gradually lost in the surrounding parts. In this situation he requested my advice. I gave him some mild salve to dress the wounded part; I directed him, constantly to keep applied to the integuments, covering the inflamed lymphatics, some cloths wetted with the cold aqua aceti lythargiriti cum opio, to keep his arm compleatly supported by a sling, and to take some gently purgative medicine.

This he did, the inflammation gradually subsided; and the wound made by the lancet healed.

It might be suspected, that in the cases which have been related, the lancet which was employed, was envenomed; and that the absorption of virulent matter was the producing cause of inflammation: the descent of the disease, to the inferior absorbents, in the two first cases, opposes that opinion; and it is further invalidated by the observations which I shall

proceed to offer. Since the structure and functions of the absorbing vessels have become so well known, the attention of medical practitioners has been directed to their diseases, and much novel information has been acquired. That which relates to the present subject, I shall endeavour briefly to state. Physiology shews to us, that the absorbents possess much sensibility: their rejection of one kind of air, which is not evidently acrid, and their ready absorption of another, in my opinion, proves them to be endued with no slight degree of sensation. Practical observation further strengthens this opinion: the celerity with which these vessels inflame, when they have imbibed noxious matter, and the pain which is suffered in consequence, sufficiently prove this circumstance. Their frequent inflammation, in consequence of disturbance of the general constitution, may be however regarded as an additional argument. A common cold produces a painful tumefaction of their glands; and in some fevers, these parts are particularly obnoxious to disease.

There

There is another circumstance, which deserves attention; when the absorbents become inflamed, they quickly communicate this disease to the cellular substance, by which they are surrounded. Most surgeons have remarked, these vessels when indurated, to appear like small chords, perhaps of one eighth of an inch in diameter; this substance is surely not the slender sides of the vessel thus suddenly augmented in bulk, but an induration of the surrounding cellular substance, to which the irritated vessel has communicated inflammation. The formation of a common bubo, is another instance of the power, which these vessels possess, of involving the surrounding parts in their disease; at first one or two glands, are found to be inflamed; but they soon become undistinguishable, in the general inflammation of the surrounding substance. This inflammation either is dispersed, or it terminates in suppuration: and on the subsidence of the general tumour, the originally diseased glands, again become noticeable. Those frequently enormous tumours, which form by the side of the neck, further confirm the observation.

servation : enquire into the origin, and progress of the disease ; and it will be found that one or two glands were at first affected, and that the disease extended itself to the surrounding substance, of which the greatest part of the swelling is composed. This remark must be taken with some limitation, for the glands of the neck, do frequently enlarge to a considerable degree, without the surrounding cellular substance, partaking of the disease ; yet in such cases, their growth is very gradual, and unaccompanied with active inflammation. Such are the reasons, which induce me to suppose, these vessels to be very sensible and irritable ; and to possess a power of readily communicating disease to the cellular substance in which they lie.

I now wish to shew, that their inflammation, in consequence of local injury, is deducible from two causes : one, the absorption of acrid matter ; and the other, the effect of irritation of the divided tube. Of the inflammation arising from the absorption of morbid matter, every one is apprized, but that, which is the effect of irritation, has been less remarked.

When

When virulent matter is taken up by the absorbents, it is generally conveyed to the next absorbent gland; where, its progress being retarded, its stimulating properties induce inflammation; and frequently, no evident disease of the vessel through which it has passed, can be distinguished. The absorption of syphilitic, and cancerous matter, affords frequent proofs of this assertion. There are, indeed, some poisons so acrid, that the vessel which admits them inflames, throughout its whole extent; yet still the glands are principally affected. When inflammation of the absorbents, happens in consequence of irritation, that part of the vessel, nearest the irritating, cause generally suffers most: whilst the glands, being remotely situated, partake less of the inflammation. The inflammation is also of a different kind, and, I think, can be discriminated: when it arises from poison resident in the part, the gland is first indurated, and a phlegmonoid inflammation follows; but if irritation be the cause of its enlargement, the tumefaction more speedily takes place,

place, the gland is more painful in its early state, but has less tendency to suppurate; the enlargement more resembles that of the lymphatic glands of the neck, which is the consequence of taking cold.

When the inflammation arises from irritation, it will be expected, and I believe it will be found, that the continuity of the vessel will be apparent: but it does not follow, that the greatest disease, will be immediately adjoining that part, which has sustained the injury. The cases which have been related, shew that inflammatory tumours often form in the middle of the arm, and forearm; when the wound of the absorbent is at the bend of the elbow. Were it necessary, I could relate several cases, where such tumours formed from injuries done to the fingers, or in consequence of fretting ulcers of the leg. When they arise from the latter cause, it might be supposed, that some acrid matter had been imbibed, yet, I think, in that case, we should find the glands the principal seat of the disease. It has been
proved,

proved, that the absorbents frequently inflame far below the part where the vessel has sustained an injury, and where the inflammation could not be occasioned by absorption. These observations I thought it right to insert, to illustrate the cases, which have been related; and also, to excite more general attention to the diseases of these important vessels.

Of Inflammation of the Vein.

After the account, which Mr. Hunter has given of the inflammation of the vein, (in the Medical and Chirurgical Transactions) no additional information from me will be expected, nor is it perhaps required. If the wound of the vein, does not unite, an inflammation of that vessel will probably follow; which will vary in its degree, in its extent, and in the course which it pursues. One degree of inflammation, may occasion only a slight thickening of the venal tube, and an adhesion of its sides; more violent inflammation may be attended with the formation of more limited, or
more

more extensive abscesses ; the matter of which, may sometimes mix itself with the circulating fluids, and produce dangerous consequences ; or it may be circumscribed, by the thickening and adhesion of the surrounding parts, and then like a common abscess make its way to the surface. When the inflammation of the venal tube is extensive, it is, indeed, very probable, that much sympathetic fever will ensue ; not merely from the excitement which inflammation usually produces ; but also, because irritation will be continued along the membranous lining of the vein to the heart. If, however, the effect of the excited inflammation, has luckily been to produce adhesion of the sides of the vein, at some little distance from the wounded part ; the inflammation will here cease : its further transmission will by the adhesion be prevented. The effect of adhesion of membranes, in preventing the extension of inflammation along their surfaces ; is frequently apparent, and has been well explained by Mr. Hunter on another occasion. In one case, Mr. Hunter applied a compress on the inflamed

flamed vein, above the wounded part, and he thought that he succeeded in producing adhesion; for the inflammation extended no further. In those cases, where the inflammation does not continue, equally in both directions, but descends along the course of the vein, it is probable, that its extension in the other direction is prevented, by adhesion.

I have thus briefly and imperfectly transcribed Mr. Hunter's opinion, that the present Essay might not be altogether deficient in information, relative to this subject. I have seen but three cases, where an inflammation of the vein succeeded to Venæsection; they, however, confirm the foregoing observations. The vein did not in either case evidently suppurate. In the first, about three inches of the venal tube inflamed both above and below the orifice; it was accompanied with much tumour, redness and pain of the covering integuments, and much fever, the pulse was rapid, and the tongue furr'd. After the inflammation had terminated, and all tumour had subsided, the vein did not swell, when compression was
made

made above the diseased part. The second case was of a similar nature, but less in degree. In the third case, the inflammation was not continued in the course of the vein towards the heart, but extended as low as the wrist. I have no doubt, but that adhesion of the sides of the vein, was the cause, which prevented the extension of the disease, equally in both directions. The nature of a disease being known, the treatment is commonly evident. The diminution of inflammation in a vein, is to be attempted by the same general means as in other parts. As the membranous lining of the vein is continued to the heart; and as inflammation is very speedily extended along such surfaces, unless prevented by adhesion: the application of a compress at some distance from the punctured part, in order to unite the inflamed sides of the vein, appears to be particularly judicious.

I am induced to suppose, a case may occur in which the vein may suppurate, and in which, a total division of the tube may be proper

proper practice; not merely to obviate the extension of the local disease, but to prevent the mixture of collected pus with the circulating fluids.

Inflammation of the Fascia of the Forearm.

As far as my observation hath extended, the next frequent ill consequence, which succeeds to Venæsection, performed in the arm; is an inflammation of the subjacent Fascia. When this complaint occurs, it perhaps arises not merely from the contiguity of the fascia to the punctured and irritated parts, but it is probable, that it was wounded by the lancet in the operation. I hope that the cases, which I shall relate, and those, to which, I can refer the reader, will convey sufficient information of the symptoms, and effects of this disease.

CASE.

A man, aged 40, was admitted into St. Bartholomew's Hospital, under the care of Mr. Pott: he had much pain and difficulty

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of

of moving, his arm, in consequence of inflammation succeeding to phlebotomy. The wound inflicted in that operation, was not healed; the surrounding integuments were not much inflamed, but he could neither extend his forearm, nor his fingers without great pain. The integuments of the forearm were affected with a kind of erysipelas; when slightly touched, they were not very painful, but when more forcibly compressed, so as to affect the inferior parts, much pain was suffered. The patient complained of pain, extending towards the axilla, and also towards the acromion; but no tumour of the arm, in either direction, was perceptible. A poultice was applied to the arm, opium was given at night, and aperient medicines were occasionally prescribed. The pain in the arm increased, and it was attended by much fever. After a week had elapsed, a small and superficial collection of matter took place, a little below the internal condyle; this being opened, but little pus was discharged: and scarcely any decrease of tumour or pain followed. About ten days afterwards,

terwards, a fluctuation of matter was distinguished below the external condyle; an incision was here also made, which penetrated the fascia of the forearm. Much matter immediately gushed from the wound, the swelling greatly subsided, and the future sufferings of the patient, were comparatively, of little consequence. This opening, was however, inadequate to the complete discharge of the matter; which, had probably been originally formed beneath the fascia, in the course of the ulna: its pointing at the upper part of the arm, depended on the tenuity, and comparative non-resistance of the fascia, at that part. The collected pus descended to the lower part of the detached fascia, a dependent opening for its discharge became necessary, after which, the patient recovered, without any circumstance being observed, worthy relation. The case which I have just related, and that, in which, two large abscesses had formed, attended with indurated absorbents; occurred nearly at the same time, at the Hospital: and they both fell under the care of Mr. Pott. In the lec-

tures of that eminent surgeon, I had heard, dangerous and fatal consequences attributed to the injury of a nerve, in Venæsection, but I learned no other distinction of cases. These cases, first excited my attention to this subject; and as far as I know, such discrimination as that, which I now offer to the public, hath not been attempted.

I saw one other case of inflamed fascia, but I neglected to take notes of the symptoms; I therefore can only say, that at the time, they appeared so clearly to characterize it, that I entertained no doubt of its nature. No inflammation of the vein or absorbents appeared, the integuments were not much affected; but the patient complained, that his arm felt as if bound, or compressed; and that he suffered much pain if he attempted to extend it. The inflammation subsided without the formation of matter; and after much time had elapsed, the pliability of the arm was gradually regained. I the less regret my deficiency of experience, on this subject, as I can refer the reader

to

to the second volume of the Medical Communications; he will there, meet with two cases, which I believe he will acknowledge to be inflammations of the fascia: attended however, with some peculiarity of symptoms.

The first case is related, by Mr. Colby, of Torrington in Devonshire; the other, by Mr. Watson. The inflammation of the fascia, in the latter case, was followed by a permanent contraction of the forearm. From this case, I think we have acquired useful knowledge: should a similar contraction of the forearm, from a tense state of the fascia, in future occur, it seems reasonable to suppose, that it may be completely relieved, by detaching the fascia, from the tendon of the biceps; to which, it is naturally connected. This, I conclude, was the cause of the perfect restoration of free motion, in the case first related by Mr. Watson. On this subject I will not enlarge, but submit the opinion to the judgment of the reader.

The treatment of an inflamed fascia, the consequence of Venæsection, has in it, no peculiarity. Doubtless, those general means which are reductive of inflammation, should be employed. Of local treatment, quietude of the limb, and a state of relaxation of the inflamed part, will tend to lessen disease; but as soon as some abatement of inflammation is procured, the extension of the forearm and fingers, ought to be attempted, and daily performed: to obviate that contraction, which might otherwise ensue.

Of the ill Consequences, succeeding to a wounded Nerve,

In order to compleat in some degree, this Essay, I have attempted to discuss the present subject; tho' I acknowledge, I have no practical information to communicate. I believe, these accidents, to be of rare occurrence, since those of my medical friends, to whom I applied for information, had never seen a case, the symptoms which they could decisively, pronounce to arise merely from an injured nerve. Mr. Pott, in his lectures, used to say, that he
had

had seen two cases, in which, the patients had suffered distracting pain, which was followed by convulsions, and other symptoms, which could only be ascribed to nervous irritation. He attributed these effects, to a partial division of the nerve, and recommended its total division, as a probable remedy. Doctor Monro, I am informed, relates similar cases, in which, such treatment has proved successful. I rely on the discrimination of these eminent men; yet I feel convinced, that the greater number of surgeons, have been deficient in distinguishing these diseases. A wounded nerve, acting as a cause, must always produce specific, and characteristic symptoms and effects. I need not insist on the necessity of discrimination, in these complaints: those, who have described the symptoms, resulting from an injured nerve, have represented them, as at all times imminently hazardous, and frequently fatal. An operation is here demanded; from it, we have reason to expect immediate mitigation of the patient's sufferings; and his future perfect restoration. Yet this operation, in any other

of the complaints, before treated of, would be unnecessary, and perhaps detrimental.

I shall arrange, what I have to say on this subject, in the following manner : First, I shall explain, what nerves are exposed to injury. Secondly, I shall investigate, what are the effects likely to be produced by such an accident ; and thirdly, I shall enquire, what means, are most likely to afford relief.

First, the two cutaneous nerves are those, which are exposed to injury, I dissected them, in several subjects, with attention ; and found some irregularity in their distribution : most frequently, all their branches pass beneath the veins, at the bend of the arm ; but sometimes, altho' the principal rami still go beneath these vessels, many small filaments are detached before them, which it is impossible to avoid wounding, in phlebotomy. As I believe, many surgeons retain but an indistinct remembrance of these nerves, and as I have never seen them accurately depicted, in any anatomical book; I thought I should do an accepta-

acceptable service, by giving an engraving of them. I therefore, made two drawings of them, one, exhibiting their most simple course; the other, their most complicated distribution. These, I conclude, are the only nerves liable to injury: it may be suspected, that the median nerve might occasionally be wounded; but its situation, I think, makes this opinion improbable. If, however, a doubt should be entertained on this subject; an attention to symptoms, will soon dispel it; when a nerve is irritated, at any part, between its origin and termination; a sensation is felt, as if some injury were done to the parts, which it supplies. If therefore, the cutaneous nerves were injured, the integuments of the forearm would seem to suffer pain; but if the median nerve was wounded, the thumb, and two next fingers would be painfully affected.

By referring to the plate, it will be seen, that if the patient be bled in the vena mediana basilica; the branches of the internal cutaneous nerve, are exposed to injury: or, if the vena mediana cephalica be opened, the branches of the external cutaneous nerves, may be wounded.

Secondly,

Secondly, I wish to enquire, what are the ills, likely to arise from a wounded nerve.—Whoever reflects on the wonderful minuteness of the nervous fibrils, and considers their perfect distinctness, from each other, although connected by a common covering of cellular substance; will scarcely imagine, a partial division of a nervous fibril. If I sought, to express myself strictly on this subject, I should speak of a partial division of a paquet of nerves. But I shall use the commonly adopted language, and call those chords, nerves; which are really composed of multitudes of separate nerves. I first beg leave to examine the opinion, which has prevailed, of a nerve being partially divided. Admitting that a nerve be partially divided, would it not, like a tendon, or any other substance unite? I think, there can be no doubt, but that it would: I am induced to this opinion, by considering, that nerves of equal size, with the cutaneous nerves of the arm, are distributed in considerable numbers, throughout the body. In the many operations performed, and in the wounds daily

daily occurring, I think it would be strange, if a partial division of a nerve, should not happen; yet, no peculiar symptoms, are observed usually to ensue. The pain which some people suffer from bleeding, in my opinion, indicates an injury done to a nerve. If the reader refers to the plate, he will perceive, that in some cases, it is impossible to avoid dividing branches of nerves, in phlebotomy: as sometimes they pass before the vein. I believe these to be branches so frequently wounded, that I should be surpris'd, if they did not, many times, suffer a partial division. Surely, however, a half divided nerve would unite without causing a general derangement of the nervous system. Yet it is possible, that an inflammation of the nerve may accidentally ensue; which, would be aggravated, if it were kept tense, in consequence of imperfect division. In the cases, related by Mr. Pott, and Doctor Monro, I believe, that some days elapsed after the receipt of the injury; before any alarming derangement of the nervous system, ensued. Inflammation of the surrounding parts, also appeared. These observations, make it to me evident, that the disease consists

sists in inflammation of the injured nerve, in common with the other wounded parts: and this inflammation, I can conceive to happen, with or without, a total division of the nervous chord. I should consider, a case of inflamed nerve, as an object of great curiosity; every one, I think, will admit, that it is likely to communicate dreadful irritation to the sensorium: and every one will perceive, that a cure will probably arise from intercepting its communication, with that important part.

Thirdly, I proceed to enquire, what is the most probable method of relieving the effects, arising from an inflamed nerve. The general opinion is, that the nerve is only partially divided; and that, a total division would free the patient from the continuance of his sufferings. Mr. Pott supposed, that the wounded nerve, was situated at one or the other extremity, of the wound, which had been made in the vein: he therefore proposed, to divide it totally, by enlarging a little the original orifice. It is however possible, that the point of the lancet might

might injure a nerve, lying beneath the vein. This will be easily understood by referring to the plate. Mr. Bell directs, an extensive transverse incision, to be made through the original wound: but if the injured nerve, be situated, at the upper, or lower extremity of the orifice, it will remain unaffected by this operation. Mr. Bell also advises the incision to be continued to the bone; but this appears to me, dangerous and unnecessary.

If the injured nerve be inflamed, I think it doubtful, whether, even a total division of it, at the inflamed part, would effectually relieve the general nervous irritation, which the disease has occasioned. To intercept the communication of the inflamed nerve, with the sensorium, does however promise perfect relief. This intention, can only be accomplished, by making a transverse incision above the orifice in the vein. The incision, need not be very extensive, for the injured nerve, must lie within the limits of the original orifice; and it need only descend as low as the fascia of the

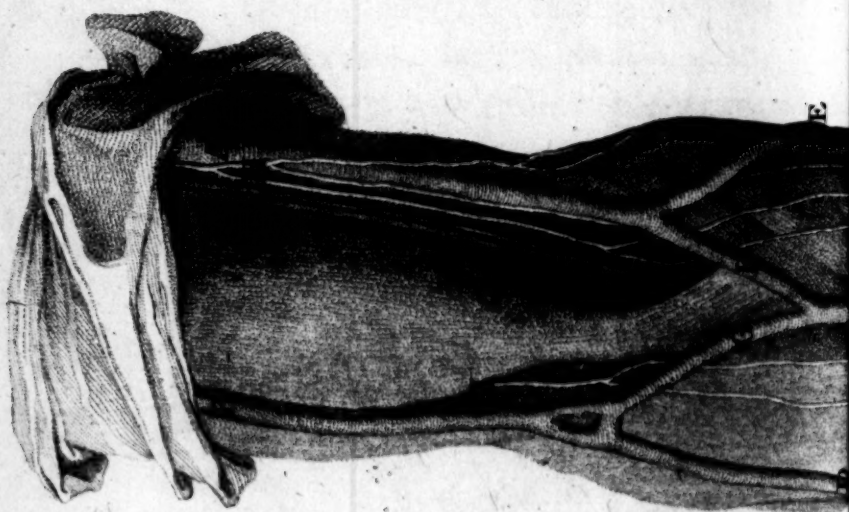
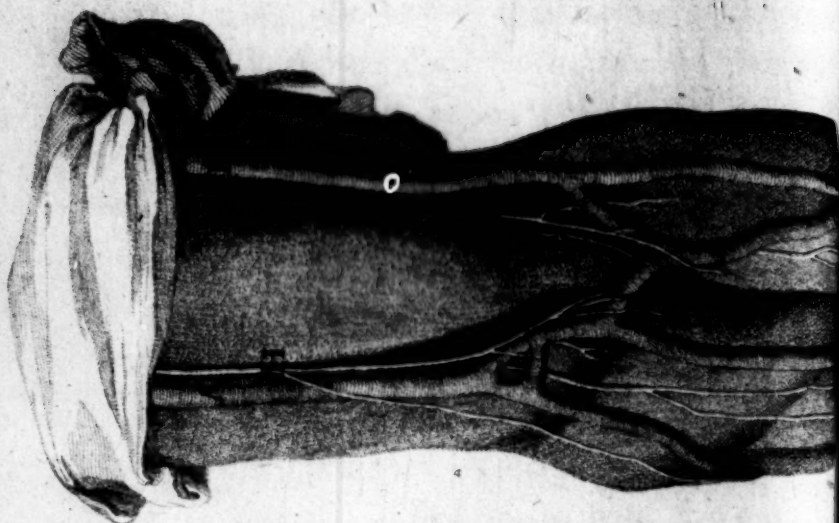
forearm;

forearm; for all the filaments of the cutaneous nerves, lie above this fascia. The vein which had been opened, and some filaments of the cutaneous nerves are all the parts of consequence, which will be divided in this operation. The proximity of the division of the nerve, to the vein, must be regulated, by the supposed extent of the disease. However, as the extent of the inflammation of the nerve, is uncertain, I submit it, to the consideration of surgeons; whether, it may not be adviseable, in some cases, to divide either of the cutaneous nerves, still more remotely from the injured nerve.

I find little difficulty, in detecting the trunk of these nerves, in the dead subject; and I should suppose, but little would occur in the living state, for the compression of the tourniquet, would prevent any obscurity, which hæmorrhage might cause.

Explana-





Of the various causes which may produce the inflammation of the cellular substance, and the manner in which it is propagated.

I think it very probable, that these diseases would less frequently happen, did not the

Explanation of the third Plate.

A Vena basilica.

B Vena cephalica.

C Vena mediana.

D Vena radialis.

E Vena cubitalis.

F Vena mediana basilica.

G Vena mediana cephalica.

H Nervus cutaneus internus.

I Nervus cutaneus externus.

*General Observations on the ill Consequences, sometimes
succeeding to Venæsection.*

I think it very probable, that these diseases would less frequently happen, did not the situation of the veins, usually opened, contribute to their occurrence. The common offices of life, so constantly demand the employment of the arm, that its motion becomes almost inevitable. Unless, the orifice made by the lancet, has been attentively closed; the effect of this motion, will be, to separate the edges of the wound from each other, and to prevent their union by the first intention. Some slight degree of inflammation will ensue; the continuance of motion of the arm, causes a friction of the inflamed surfaces against each other; and thus, the disease is increased. Under these circumstances, if the constitution of the patient be irritable, the inflammation will extend itself; altho' it may still be confined to the cellular substance, and integuments: or perhaps, it may be transmitted to that part, which has sustained most injury

injury in the operation. The vein, the absorbents, the fascia, or the nerve may in that case, suffer peculiar derangement. Although the injury done by a bad lancet, may contribute to the production of disease, yet I think it probable, that a patient improperly bled, would sustain no injury, if the treatment of the wound was judicious. Whilst another, on whom, the operation had been dexterously, and well performed, would be liable to these ill consequences; if the proper attention to unite the wound, was neglected.

In the account given of these diseases, they have been represented, as they occurred, separately; doubtless, in some cases, they may be combined.

The principal curative indications, appear to be, to mitigate the inflammation about the orifice, and to preserve the arm, supported in a motionless state. I need not enlarge this account, by describing the modes of appeasing inflammation, and irritation; as they are well known, to every surgeon.

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I cannot conclude these Essays, without offering to the public, an apology for their defects: whoever, by diligent attention, has acquired a competent knowledge of his profession; if he continues those exertions, by which, he has attained his present information; will extend his pursuits, beyond the former boundaries of knowledge. New modes of treatment, which promise success, he will be induced to practice: whatever appears doubtful, or obscure, he will endeavour to ascertain, and illustrate. His labour will be profitable to himself, for he will acquire the information, which he has sought. A person engaged in the pursuit of useful science, ought not here to stop, but should communicate the information, which he has obtained. If indolence, or diffidence of success, or a wish to attain perfection, induce him to postpone the communication of his newly acquired knowledge; his attention will probably be attracted by new objects of investigation: the remembrance of his former engagements, will fade in his mind, and the effects of his exertions,

exertions, will be less profitable to himself, and of little service to others.

Such reflections, have induced me to offer, that, which I knew to be deficient; rather than to suppress information, which I believed might be, in some degree, useful. The imperfections, and defects of these Essays, will, I trust, be forgiven; if it be found, that they have added any thing, to the general stock of knowledge: and I shall feel my ambition gratified, in having contributed my mite, to the advancement of the eminently useful art of healing.

